SCIENCE

FRIDAY, APRIL 30, 1897.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE NATIONAL ACADEMY OF SCIENCES.

THE annual meeting of the National Academy of Sciences in Washington began on Tuesday, the 20th inst., and continued through the following Wednesday and Thursday.

The meeting was remarkable for the unusually small number of scientific papers presented and the unusually large number of obituary notices and biographical memoirs read.

In the order of their presentation the scientific papers were: 'An Experimental Study of the Influence of Environment upon the Biological Processes of the Various Members of the Colon Group of Bacilli,' by Adelaide Ward Peckham (presented by Dr. Billings); 'On the Energy involved in Recent Earthquakes,' by Dr. Mendenhall, who, with Professor A. S. Kimball, presented a second paper on 'A Ring Pendulum for Absolute Determinations of Gravity,' the first being discussed at some length by Professors Agassiz, Rowland, Gilbert, Hastings and Mendenhall; 'On the Variation of Latitude,' by Dr. S. C. Chandler; 'On the Position of the Tarsiids and Relationship to the Phylogeny of Man,' by Dr. Theodore Gill; 'A New Harmonic Analyzer,' by Professors Michelson and S. W. Stratton; 'A report on the Variation of Latitude and Constant of Aberration from Observations at Columbia University,' the joint work of Professors Rees and Jacoby and Dr. Davis, presented

by Dr. S. C. Chandler; 'On Recent Borings in Coral Reefs,' by Professor Agassiz; and 'Notes on Experiments upon the Röntgen Rays,' by Professor Arthur W. Wright.

Biographical memoirs were read as follows: Of Dr. G. Brown Goode, by Professor Langley; of General Thos. L. Casey, by General Abbott; of Dr. Brown-Sequard, by Dr. Bowditch; of Professor H. A. Newton, prepared by Professor J. W. Gibbs and read by Professor A. W. Wright; and of Professor George H. Cook, by Mr. G. K. Gilbert.

Formal announcement was made of the death, since the last meeting of the Academy, of four of its members, including several of those most eminent and most widely known, namely, Dr. B. A. Gould, General F. A. Walker, Professor E. D. Cope and Professor M. Carey Lea.

Four new members were elected: Professor Morley, of Cleveland; Dr. Minot, of Boston; Dr. Dall, of Washington, and Professor Gooch, of New Haven.

On Wednesday Professor Asaph Hall was elected as Vice-President, to succeed General Francis A. Walker, who filled that office at the time of his death. Professor Remsen was elected to succeed Professor Hall as Home Secretary, and Professor A. Graham Bell was chosen as Treasurer of the Academy, to succeed Dr. Billings, who resigned that office, owing to his removal to New York.

Sir Archibald Geikie, Director of the Geological Survey of Great Britain, who is giving a course of lectures at the Johns Hopkins University, was the guest of the Academy at luncheon on Thursday, and was formally presented to the Academy at the session immediately following.

The plan agreed upon a year ago, of having the reading of papers begin at 2 p. m. and not attempting scientific work in the morning, was followed this year and will be productive of good results as soon as it is generally understood. It may be assumed that the Academy is indifferent as to whether it has an audience or not, and it is becoming quite evident that those who do not belong to it are pretty much of the same mind; but it is none the less a fact that there are always many people in Washington at the time of its annual meeting, including many residents and many temporary visitors, who would be glad to listen to the papers and discussions, and as long as the so-called 'scientific sessions' are avowedly open to the public, an invitation to attend being in some sense offered, some consideration ought to be given to those who often put themselves to much trouble to be present. The new plan of having a definite hour for beginning the 'scientific session' is an important step in this direction, and it ought to be followed by a rigid adherence to the order of the printed program in the presentation of papers. Often a member will invite those specially interested to be present at the reading of a particular paper, only to be disappointed by a change in the order to accommodate one whose title is lower down on the list. The Council has full power, it is assumed, to arrange the list as it deems best, but when once printed it should be adhered to, certainly, unless departure from it is by common consent.

The Academy is not only indifferent to the presence or absence of listeners, but it is probably equally indifferent to criticism from the outside; but having already ventured a word of critical comment, it will do no harm to add another and remark that if the formal introduction of a distinguished foreigner is an event likely to occur again it would be well to have some understanding among the few members who may accidentally be present at the time as to whether they are or are not expected to share in any way in the bestowal of this pleasant compliment.

On such occasions, and, indeed, on all occasions when the Academy is in session for other than the transaction of its private business, the presence of a goodly number of its members would be desirable, and it ought not to forget that it is a *National* Academy, chartered by the government; therefore to a degree the creature of the people and their representatives in the highest domain of scientific investigation. They do not wish to direct or restrict its operations, but are content to see that they are controlled by a membership which includes the ablest specialists which the country produces, selected from time to time in accordance with a standard with which they have no particular quarrel. On the other hand, the Academy may well give great consideration to its obligations to such an enormous and unusually intelligent constituency, whose character and dignity, from the scientific standpoint, it is delegated to represent.

The November meeting will be this year in Boston, beginning on Tuesday, the sixteenth of that month.

AN ESSAY ON THE CLASSIFICATION OF INSECTS.

OF late years the phylogeny of insects has attracted considerable attention from students, and much light has been thrown upon the subject by the researches made. One of the most notable facts has been the breaking away from the old Linnæan orders and the substitution of a number of more compact assemblages for some of the almost indefinable aggregations found in the old classification. New characters have been sought, not only in structures visible externally, but even in internal anatomical peculiarities. The subject is a very interesting one, which the teacher is of necessity compelled to study more or less, and which I was led to examine more particularly when the question recently came up as to the adoption of some system in a general work on 'Economic Entomology,' which has since been published. The conclusions reached by myself, while in general they agree with the latest published results, have been arrived at by a somewhat different method, and my ideas concerning the development of the orders are somewhat unlike those heretofore accepted. I have tried to adhere logically to a scheme of easy development, and have made use of some characters not heretofore particularly noted. Leaving aside for the present all questions as to the origin of the class 'Insecta' and as to its ancestors, I start from a developed hexapod—an archetypal Thysanuran with six, jointed legs; without wings; with or without abdominal appendages other than functional legs; with no eyes or with ocelli only; with a head not greatly differing in size or form from the body segments; with the thoracic segments equally developed and not greatly differing except in appendages from those of the abdomen. This creature lived in moist places, perhaps partially in the water, and had the tracheal system feebly or not at all developed; absorbing oxygen chiefly through the skin and tending, perhaps, as much in the direction of an aquatic as a terrestrial life. It had no distinct metamorphosis, was oviparous, bisexed, changing little in appearance from the time it emerged from the egg until it was adult and capable of reproduction. The mouth structures were generalized, feebly developed; but with at least three, and possibly four, pairs of composite structures corresponding to mandibles, maxillæ and labium of our existing insects. The possible fourth pair may have been an endo-labium and, perhaps, the labrum with its attached epipharynx may have required a fifth pair of structures. Most essential of all was an inherent power of variation and adaptation, and probably, as with some of our present Thysanurans, reproduction was

rapid and enormous numbers existed. The first important differentiation occurred in the mouth structure long before wings became developed, tending on the one hand to a perfection of all or most of the parts, or to a mandibulate type; on the other to a loss of certain of the structures, accompanied by a different development of the others, forming a haustellate type. In this latter branch the mandibles were never developed, the maxillary structures became elongated, separated into their parallel parts, and the labium became obsolete as a functional organ. Just how many intervening orders existed between *Thysanura emandibulata* and the best development of the haustellate structures it is impossible to say; but the only one in existence at the present time is *Thysanoptera*, also called *Physopoda*, otherwise *Thrips*.

This order I consider a distinct one on the same branch from which arose the *Hemiptera*, but forming merely a short spur and retaining characters which were soon lost in the main and more vigorous branch. It is a survival which has lost the power of further development, and can do no more than merely maintain itself. The main branch formed the *Hemiptera*, or, as I prefer to call them, the *Rhyngota*, of to-day; the mandibulate parts being completely lost, the labium losing all external appendages, and the maxillæ forming the jointed beak with its inclosed lancets.

The *Thysanoptera* and *Rhyngota* of all the existing orders are the only ones that do not have functional mandibles in some stage of their development. They are haustellate from their birth, and the character of the mouth parts never changes. In all the other orders, either larvæ or adults, or both, are mandibulate. I am aware that there are seeming exceptions in several orders, notably the *Diptera;* but it will hardly be disputed that this order is of a mandibulate stock, and many larvæ have the parts well developed.

It results from the views just stated that the *Thysanoptera* and *Rhyngota* are a division equal in value to all the other, or mandibulate, orders combined. They have their origin from the common stock; but were always haustellate or emandibulate in all stages, forming the first and lower of my main divisions. With the development of this branch, after its distinctive feature became established, I have nothing to do at present. It seemed adapted for variation in special lines only, and, as the method of feeding was practically fixed from the beginning, there is a remarkable similarity in mouth parts throughout.

The *mandibulata* possessed much greater powers of variation and a mouth structure in which all the parts were developed and capable of modification, containing possibilities of much greater range in obtaining food. They lived, therefore, under all sorts of conditions, in all sorts of media, and all kinds of modification were produced; some of them short-lived, adapted only to surroundings then existing; others with greater possibilities, that exist to the present time.

The first mandibulate insect had the thoracic segments similarly developed, all of about the same size and each of them free; but the advent of wings gave opportunity for radical divisions. I have no desire to go into details here more than necessary to explain my views of classification, hence will not pretend to account for the origin or development of wings. They did appear, however, and independently at several different points. In all cases the wings were net-veined or neuropterous in type, a peculiarity which is explicable if the venation be considered of a tracheal origin. With the appearance of wings many divergences in habit were made possible and new types began to appear. Three main lines branched almost simultaneously from the common stock, each of them fairly well marked from the beginning, retaining its

peculiarities and even intensifying them in all future subdivisions to the present time. In the first of these the prothorax, bearing no wings, became separated from the other rings and movable, or in a sense dominant. In both the others it tended to a reduction in size or to become agglutinated with or united to the others. In a general way it may be said that the series in which the prothorax is free is lower in the scale of development, as retaining a more primitive type. The orders belonging to this subdivision or branch are the *Dermoptera*, *Coleoptera*, *Plecoptera*, *Platyptera* and *Orthoptera*.

If we examine this series as a whole several characters will be found to challenge attention: First, a series of similarities in the mouth structure. Omitting the *Coleoptera* and *Platyptera*, which are most highly specialized, all the others agree in the general structure of the labium. In the Earwigs, Stone-flies and Roaches a divided ligula is quite usual, and throughout the *Orthoptera* glossa and paraglossa are usually separate and even jointed. In the maxilla of all the orders the lacinia may be said to dominate and the galea tends to become rather a subordinate, often palpiform structure. There are numerous exceptions to this in the *Coleoptera* to answer special requirements, but I believe that, as a whole, my statement is correct. The maxilla tends to the exercise of mandibular functions, and the lacinia is the sclerite armed and modified for the chief labor. Throughout this entire series of orders the head is fairly well set into the prothorax. There is no development of a distinct neck between the head and the first thoracic segment, and in many cases the head can be almost entirely withdrawn into the prothorax. This is an important feature which, so far as I am aware, has not been sufficiently valued. In wing-structure the secondaries dominate throughout, and the uniform tendency is to a reduction in size and loss of function in the primaries. Furthermore, the wings lie flat upon the back, and the secondaries are folded under the primaries. To this structure of the wings, and the method of carrying and folding them, I attribute much weight, for it seems to me that, combined with the other characters of head and thorax, it argues a community or origin and a separation from those forms differing in these features.

Among the most primitive in this series are the *Orthoptera*, of which the roaches and walking sticks are the most generalized in mouth structure as well as in the way the wings are carried. In this order the dominance of the secondaries as organs of flight is established, and the tegmina or primaries are more and more changed in character. The hind wings are always folded longitudinally under the primaries and sometimes both pairs are lost. In the primaries a gradual change in position occurs, part of the wing being first bent down in the crickets to protect the sides, the character becoming more prominent in the Locustidæ and most obvious in the Acrididæ which, in my opinion, are the highest of the order in point of development. Some of the roaches have the wings folded transversely as well as longitudinally, and this is a very primitive character which emphasizes the relation of these insects to the *Coleoptera* and points to a common ancestor.

A prominent feature in the *Dermoptera* and *Coleoptera* is that the secondaries are transversely folded, separating these orders at once from all the others except the few roaches already mentioned. It is, of course, true that there are *Coleoptera* in which the secondaries are not transversely folded; but these are secondary peculiarities and exceptions to the rule. I am inclined to attribute considerable importance to this character, and to give these orders an independent derivation from a Thysanuran

spur, very close, however, to the point from which the roaches originated. The *Dermoptera* cannot remain associated with the *Orthoptera* and present more affinities to the *Coleoptera* from my point of view. I do not mean to say that the Earwigs were the ancestors of the beetles; but that both were derived from the same spur in which the secondaries became transversely folded, and the *Dermoptera* now present some of the essential characters of the ancestral Coleopteron. The *Coleoptera* proved a vigorous shoot and stand far the highest of all those series with a freely movable or separate prothorax.

While the terrestrial branches were developing independently, two aquatic types, the *Plecoptera* and *Platyptera*, became developed, the larval forms living similarly under the surface of the water, but assuming a winged, aërial type before becoming capable of reproducing their kind. The *Plecoptera* or *Stone-flies* have the metamorphosis incomplete, while the *Platyptera* have it complete. The differences in this respect are very slight, however, and I have no hesitation in classing these forms together as comparatively small divergences from one stem. It will be noted that I use the term *Platyptera* in a different sense from any in which it has been heretofore employed, and do not include with it either the *Chrysopidæ*, *Hemerobiidæ* or *Myrmeleonidæ*. Raphidia and Mantispa, which seemed at first sight referable to this series on account of the elongate prothorax, do not really belong here, because this segment is not free, but is closely united at its base with the mesothorax. The *Plecoptera* are, of course, much the most primitive and are a survival, the main line of development continuing in the direction of the *Platyptera*.

The second branch from the Thysanuran stem started with all the thoracic segments nearly equally developed. While the prothorax was of good size and in the lowest forms quite free, yet the tendency was from the very start to unite it at its base to the other thoracic segments. In this series it is always fairly well developed, sometimes even very long; but it is always closely joined to the meso-thorax at the base and is not movable, while the tendency is for the head to become free from it, and at all events not to be inserted into the thoracic segment. While we do not have anywhere in this series a distinct neck, yet on the other hand there is nowhere a retraction of the head into the prothorax. In this series both pairs of wings are similarly developed, both as to size and as to general character, while the secondaries, though frequently covered by the primaries, are never folded beneath them in any way. The primaries are always functional.

The lowest in this series, and almost the simplest in general structure, are the *Isoptera*, where all the thoracic segments are well developed and the prothorax is scarcely dominant, though larger and almost free from the others. The wings are very much alike, the secondaries only a little larger than the primaries, and both are laid flat upon the abdomen. The mouth structures are almost identical with those of the earwigs and some of the *Orthoptera*. I believe the members of this order are among the most primitive of all the terrestrial winged insects now existing, and among the most ancient, though remarkably specialized in certain directions at the present time. Though at first glance it would seem as if these insects should belong to the series in which the prothorax is free, yet the character of the wing structure forbids this association and makes the *Isoptera* a natural stem from which were derived the *Mallophaga*, *Corrodentia* and *Neuroptera*.

The *Mallophaga* are a degraded parasitic type which were not improbably developed from a wingless Isopteron, and perhaps at about the time that the wingless forms of

the *Corrodentia* were also developed. The *Corrodentia*, and especially the winged forms, are peculiar in many respects and stand by themselves; but I believe that they are derivatives from the branch upon which I have placed them. I do not consider it at all improbable that in the *Corrodentia* wings were independently developed, and indeed cannot well explain the peculiar venation on any other theory.

The *Neuroptera* are evidently derivatives from the *Isoptera* stock. Here we have the prothorax well developed in all cases, sometimes very long indeed, but always united at the base to the meso-thorax and never movable. The wings are similarly developed, both pairs used in flight, the primaries covering the secondaries, but neither pair folded in any way. All the forms are terrestrial, as indeed are all belonging to this branch. In all cases the larvæ are predatory and have a similar appearance, in the younger stages at any rate. I exclude the *Sialidæ* from this order, because of the movable prothorax and the folded secondaries, and include of our American families only the *Mantispidæ*, *Chrysopidæ*, *Hemerobiidæ*, *Myrmeleonidæ* and *Raphidiidæ*. This branch is one of fragments, and all the groups belonging to it, or orders, if we choose to call them so, are of small extent. They may be considered remnants, and the branch as a whole does not seem to be increasing at the present time. It will be noted that as at present constituted it contains no aquatic species. Its point of origin, therefore, is very close to that from which the *Orthoptera* and *Coleoptera* branched.

The third series, in which the prothorax becomes much reduced in size and firmly articulated to the meso-thorax, has the body parts as a whole much more closely jointed and globular. The tendency is to bring the origin of the legs close together, and to the loss of the sternum as a distinct part or sclerite between the coxæ. The mesothorax becomes dominant and best developed, bearing also the chief organs of flight. As a whole, subject to many exceptions, the tendency is to the development of the primaries, which are never reduced to mere wing-covers and never lose function. The tendency seems to be rather to a decrease in the size of the secondaries, as in *Hymenoptera*, and to their total loss, as in the *Diptera*. There is, however, a great deal of variation in this respect, and the most that can be justly said is that in this series the secondaries never become the only, or primary, organs of flight. Another point of very great importance is that here the head is nearly always more or less free or well separated, tending to the formation of a distinct neck; while there is never any insertion of the head into the prothorax. This fact will become very striking when the orders that are placed here are compared with those in the other section, and this difference in the articulation of the head has never been, in my opinion, sufficiently emphasized in our classification of the orders. It is closely correlated with the decrease, in size, of the prothorax.

In mouth structure the tendency is all in the direction of galear development in the maxilla, while the lacinia becomes constantly less important. In the *Diptera*, in which this series finds its highest development, the galea predominate over all other mouth structures. In the *Hymenoptera* the galea is always most highly developed, and particularly so in the bees, the most completely differentiated of all in the order. In the *Lepidoptera* the galea alone is developed into a functional organ, and in those net-veined orders in which the mouth parts are not rudimentary merely the galea is at least as well developed as and never subordinated to the lacinia. The orders in which I placed in this series are *Odonata*, *Ephemerida*, *Trichoptera*, *Mecoptera*, *Hymenoptera*, *Siphonoptera* and *Diptera*.

The *Odonata* presents the characters of the series in a very compact form and evidently had an early origin, though now quite decidedly specialized. As they exist at present they are the end of a very distinct line, once much more numerous than they are now, and they show us a survival of one of nature's experiments in methods of reproduction. The separation of the copulatory organs from the testes is a unique character for which some cause must have existed. I am aware that elsewhere similar separations exist, but I am not acquainted with any similar character in the *Insecta*. At all events the line leading to the Dragon flies was single, and none of our existing orders lead to it.

The geologic record, and their loosely jointed make-up, point to the *Ephemerida* as the most primitive in this series; but even here we have, well-marked in most of the forms, the free head, fairly distinct neck, unimportant prothorax, always closely joined to the meso-thorax, and the dominant primaries. The order has not varied much and is a survival; but from the same stem bearing the *Ephemerida* all the other orders of this branch have originated, giving them all a derivation from an aquatic larval type.

As the earliest spur from this branch we have the *Trichoptera*, in which the larvæ remain aquatic, but have assumed a cylindrical, caterpillar-like form, and from these the *Lepidoptera* were derived in comparatively recent times. The break between these two orders is not very great even at present, and in many of the *Lepidoptera* characters of a Trichopterous type may yet be distinguished.

The *Mecoptera* branched from the same stem with the *Trichoptera* with similar worm or caterpillar-like larvæ, some of which were probably aquatic; others lived in mud or moist ground, where some of them are still to be found, while yet others became entirely terrestrial. From one of the semi-aquatic forms the *Diptera* were derived. In the adult *Mecoptera*, instead of a loss of mouth parts, which was the tendency in the *Trichoptera*, we had rather a development of all the parts in parallel series, much as in *Panorpa*, which even at present retains many of the primitive characters.

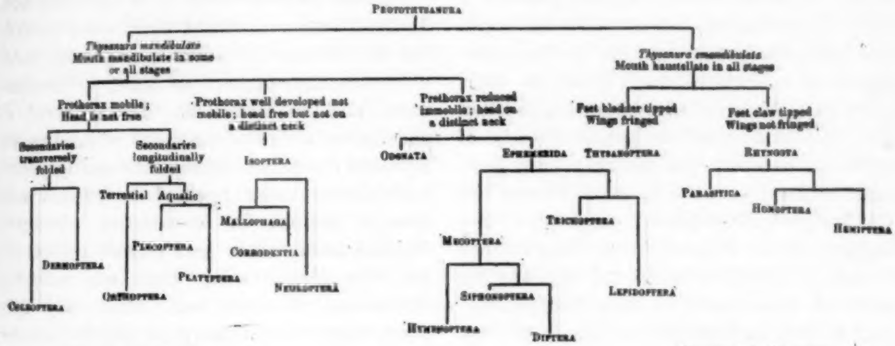
I am inclined to give the phytophagus *Hymenoptera* a much earlier origin than the *Diptera* and to derive them from the Mecopterous branch before it became very highly specialized. The *Diptera* seem to me to be the most recent of all the insect lines, and embody the highest type of that series in which the thoracic rings are united. Here the head is entirely distinct, the prothorax firmly united with the other rings, which are, themselves, solidly joined. The forewings dominate to the exclusion of the secondaries, and the galear structures of the mouth are the most highly specialized, showing, however, when closely studied, a remarkable resemblance to those of the *Hymenoptera* and pointing very strongly to a community of origin.

The *Siphonoptera*, or fleas, are entitled to ordinal recognition. They have much in common with the *Diptera*, but a type of mouth structure which could not possibly have been derived from the type now existing in that order. There is nothing, however, to prevent the belief that they developed from the same Mecopterous branch which culminated in the *Diptera*. In fact, the mouth parts of the fleas resemble those *Mecoptera* very interestingly in certain directions, and will be, I think, best understood by comparing them with that series.

I am quite aware that objections may be urged to this scheme, and that it is imperfect in some repects, but so also are all the others that have been proposed; and I believe, as I look at the matter, that my plan answers more of the objections than any other that I have seen. Nothing known

to me contradicts it more vitally than any other that has been proposed.

I have accorded very little place to the character of the metamorphosis, because there is no hard and fast line between complete and incomplete; but the closer comparative study of early stages will unquestionably help out our future classification. I have not made use of any one character as the basis of my scheme of division, because I do not think nature works in that way, and finally, I have used adult stages only, because I see in the adult ready to reproduce, the species. It is the culmination of individual growth, and until it is ready to reproduce it is incomplete, subject to change, and not an expression of the point to which its development has attained. In another form my scheme may be expressed as follows:



RUTGERS COLLEGE. JOHN B. SMITH.

*HOW MAY MUSEUMS BEST RETARD THE ADVANCE OF SCIENCE?**

VARIOUS subjects have at various times suggested themselves to me as appropriate for a paper to be submitted to this Association, but when I read the magnificently exhaustive address by Dr. Brown Goode, published in our last Report, it was manifest that all the ideas I had ever had were anticipated in that masterly production. There is, however, one side of our subject which has hardly had the attention paid to it that it undoubtedly deserves. We have been taught how best to arrange our museums for the satisfaction of the collector, of the student, of the investigator, or of the British public, but no one has ever pointed out to us the magnificent opportunities that are at our disposal whereby we may accomplish the great work of retarding the advance of science. It will perhaps not be wholly waste of time if we devote a few minutes this morning to considering this great power that is in our hands and how we may avail ourselves of it.

There are certain lines of conduct that are so surely and obviously prejudicial to science that the most uninstructed curator scarcely needs to be reminded of them. None of us but has been taught how to bewilder the eyes of the public with thirty specimens of an object, all placed the same way up, and displaying as few of its essential characters as possible, when one specimen properly labelled would have sufficed. We know how to strike dullness through the hearts of thousands by our funereal rows of stuffed birds with their melancholy lines of Latin names; we know how to chill the enthusiasm of the young and to disgust the susceptibilities of tender souls by the

* From Report of Museum Association for 1896.

display of entrails and abortions stewing in some brown decoction in the depth of antiquated pickle-jars. To suggest such well-known methods to the experienced audience of practical curators before me would be ridiculous and a waste of time. Fortunately there are further means that may be employed, and more subtle actions that may be performed, all tending to the same end.

First let us consider that jealousy with which a museum curator should guard the precious specimens entrusted to his care, forbidding the profane hands of the mere anatomist ever to disturb them in their holy rest. An excellent instance is afforded us by the history of the genus *Spirula*, of which an account has recently been published by Dr. Pelseneer in the Report of the 'Challenger' Expedition (Appendix, Zoology, pt. 83). Naturalists for long desired to obtain individuals of this interesting genus for dissection, but only fragmentary specimens came into their hands. At last, in 1865, a complete individual was collected near Port Jackson. The hopes of the naturalist were raised; "*but*," says Pelseneer, "but it was deposited in the Sydney Museum, and consequently could not be made the subject of anatomical research." There are other specimens in various public and private collections, notably in London; but they too, like the specimen in the Sydney Museum, cannot be made the subjects of scientific investigation. As curators we must regret that two specimens which were in the hands of a captain of the French navy, who for many years zealously refused to trust them to a zoologist—we must, I say, regret that, in the words of Pelseneer, "on the death of their owner, thanks to Professor Giard, these *Spirulæ* did not become the prey of a public collection." There are investigators so eaten up with their own conceit as to be bold enough to say that a specimen which shows nothing cannot be hurt by dissection, since it cannot show less and there is the chance of its showing more. Be not deceived! Do not allow the hidden recesses of your specimens to be explored by the devastating scalpel! What does it matter whether their internal anatomy can be seen or no? They have been entrusted to you for safe preservation, and you as a faithful steward will have to render account of the same.

The exhibition series of a museum are, in their essence, potent agents for retarding the advance of science. By mere force of circumstances, lack of time, undermanning, and so forth, the arrangement of specimens in the show-cases of a museum remains the same throughout many years, and names there applied to genera and species cannot be constantly changed. Classifications come and classifications go, but the classification that was adopted when the museum was built, say fifty years ago, seems likely to go on forever. Possibly even those who are in favor of introducing ideas into our scientific classifications, and who think that the arrangement of species and genera should be in accordance with their affinities and the facts of their structure, and, therefore, should change as our knowledge of that structure increases—even those fanatics, I say, may possibly regard this influence of museums as in some sort a chastening one. After all, it may not conduce to the advancement of science that each of us should have his own special classification and should call animals by his own pet names; and the museum here comes in, like its companion, the text-book, as a maintainer of stability amid the vagaries of ephemeral publication. Still knowledge *does* increase, science *does* advance, and classifications and names unfortunately do change. It is in our power to prevent this knowledge percolating to the mass of the people. If we are unable, like the text-

book writers, to foist upon the public senile illustrations that are nothing better than caricatures, still we can always excuse an effete arrangement or an obsolete nomenclature on the plea that we cannot possibly find the time or the money to re-arrange or re-name the specimens. We can, with much show of justice, refuse to give concrete form to the philosophic ideas of our greatest thinkers. We can refuse to allow our specimens to be experimented with, and arranged this way or that way according as a systematist may desire to check the working-out of his system.

Allied to the natural conservatism of museums is another efficacious practice. It is a well-known story that in the good old days of zoology, when species were regarded as separate creations, a profane sceptic ventured to ask one of our greatest zoologists what he really did manage to do with the connecting links. After looking carefully round the room, the zoologist whispered in reply, "My dear sir, I throw them out of the window." It is these window specimens that form the basis of our theories of evolution. It is by their means alone that we can work out the numerous problems that are pressing on us to-day—the problems of geographical distribution, the problems of heredity and growth, all the vast problems of the origin of our groups of animals. It is these window specimens that the museum curator always has suppressed with a stern hand; may he long continue to do so! How absurd it would be to expect otherwise! Under what names should we enter them in our registers? How could we place them in our cases? Where, indeed, should we find the room for the thousands of variations from the central types that are to be met with in all parts of the world? A museum, being finite, must select more or less, and if we select only those specimens that agree with the diagnoses of authors, we shall be saving both ourselves and the authors a vast amount of trouble. With regard to the numerous details valued by that exacting creature, the modern biologist—details of locality, of season, or, in the case of fossils, of the definite zonal horizon—it is hardly necessary to add that their accumulation would involve the curator in enormous labor, and if indulged in would probably lead him to the collection of an absurd number of specimens.

All that I have yet said may be summed up in the one phrase 'Shun ideas!' Would it be believed that a certain Professor Herréra, of the National Museum of Mexico, has recently produced a paper in which he says that the museum of the future is to be a museum of ideas? "There will be no gallery of birds, or of mammals, or of fishes, or of reptiles; no collection of Coleoptera, no collection of Chiroptera or of pheasants, or of pigeons. Museums of the future do not classify by classes, families, tribes, genera, species, sub-species, varieties, sub-varieties, races and sub-races; they put in order facts and classify ideas. There are rooms for heredity, for ontogenesis, cœnogenesis, variation, mimicry, the struggle for life, nutrition, and so on. These rooms are arranged in a philosophical order, and in that order they must be visited by the public; to this end there will be barriers suitably disposed. In the museum of the future the specimen is the lacquey of an idea; whereas, in our present museums, ideas are the slaves of specimens. Thus a specimen is not exhibited because it is rare or because it ought to be exhibited; we show the most profound contempt for specimens that are rare, curious or pretty. The museum of the future aims at being, not a magazine of dead lumber eaten by worms, but an open book in which men can read the philosophy of nature." And, after suggesting some ideas that may be exemplified in museums, our author concludes, "but, instead of studying these ideas and exhibit-

ing them in his museum, from time immemorial man has tried to imprison the things of nature in a fixed system, a fixed classification, which is not the whole of science, and which cannot be the nest of the whole of philosophy. Nature, in her vastness, protests against the classifiers; maddened, indignant, despairing, she revolts against routine." What rubbish! How can the curator at £70 a year be expected to have ideas of this kind? And how assuming that he has found the intelligence, how can he spare the time to put them into operation? And what would our Boards of Governors, our Trustees, our Town Councils, say if they went into a museum and found a curator, instead of mounting specimens by the hundred, and making as large a display as possible, calmly sitting at his table reading the 'Origin of Species,' or the latest number of the *Archiv für Entwickelungs-Mechanik?*

Apropos of the curator, he has been described, and very rightly, as the soul of the museum. What kind of a soul does the museum want? It is obvious that the curator should not be a scientific man; for if he be he will constantly be led astray from his work of labelling, ticketing, mounting, and so forth, to investigate the relationships, distribution, and what not, of some new species that has come into his hands; or, in tracing out some peculiar facts of anatomical or historical interest, he will waste the time that should be employed in compiling a list either of specimens figured by others or of his own grievances. The function of a curator is to keep his specimens clean, to keep them in order, and to exhibit them in such manner as will satisfy the annual visitation of his Board of Trustees or his Town Council. The motto that the curator should hold before his eyes is that famous one, 'Surtout point de zèle.' It is not for him to add to the stores of the museum by spending his Sundays in the country collecting fresh specimens, or his holidays in foreign lands to verify the localities whence specimens have come. It is not long since a paper was read before this very Association, read, I regret to say, by a person for whom I am in some respects responsible, recommending that the museum assistant should be sent out "to collect in the fields, the rocks and the seas," then that he should "study the specimens that he has collected, each of which will have for him an interest and a living history which under present conditions it never has; by their means he will extend the boundaries of knowledge and confirm the foundations of system, so that it is for him an expression of universal thought, and no longer a mechanical device for sorting species into their places. Then, with this vitalized classification, and with some real meaning in his head, he will proceed to prepare his most instructive specimens for exhibition, so that the final result may convey to others something at least of the beauty he himself has found in the world."—So too, at the beginning of the century, P. A. Latreille wrote in Sonnini's edition of Buffon ('Insectes,' I., p. x.) "L'homme, qui n'étudie les Insectes que dans son cabinet, peut être descripteur; mais il ne sera jamais, à ce que je pense, un profound entomologiste." But the curator does not require to be 'un profond entomologiste,' 'un profond géologiste,' 'un profond biologiste,' or anything that remotely resembles a scientific man. The curator should take for his pattern and exemplar the clerk in a dry-goods store.

We turn now to a certain practical detail in the arrangement of our museums, which fortunately seems to commend itself to the outside public who are not scientific people, and especially to the donors of specimens and bequeathers of collections. I mean this idea of keeping certain collections separate according as they happen to have

belonged to some person with a lengthy name, or to have been described by some pottering genius of the locality, or, perhaps, merely to have been presented by some individual, who, because his name was utterly unknown, desired to adopt this method of bringing it into prominence, and laid it down in his will that his specimens were to be known for all eternity as the 'Peter Smith Collection.' This method, at all events, places an insuperable bar in the way of our associating specimens that the student wishes to compare, and enables us to hide from the gaze of the traveling man of science specimens of historic interest that he may have come to our museum on purpose to see. Permit me here to indulge in a fragment of autobiography. Many years ago I journeyed to Strassburg on purpose to examine certain specimens that had been described by Mr. de Loriol. The various curators whom I met at the museum assisted me very willingly throughout three days searching for these specimens, but they could not be found, and I went on my way sorrowing. Arrived at Freiburg, I mentioned the fact to my friend, Professor Steinmann, who suggested that possibly the specimens might have been overlooked as being in the Cartier collection. At considerable expense and inconvenience I, therefore, returned to Strassburg, and, sure enough, there were the specimens carefully obscured. I have known instances of eminent foreigners coming to a great museum in our own country, desirous of inspecting certain remarkable specimens, and, after searching for many hours in the cases, where all logic would lead one to imagine the specimens were, learning at last that they were at the other end of the museum because they had once belonged to some vainglorious amateur, or been described by some muddle-headed genius of the dark ages. Who, after this, can say that such a system is not to be encouraged?

Somewhat akin to the distribution of specimens among various collections, and equally efficacious as a skid on the wheels of science, is the practice that still obtains in the majority of our museums of separating recent and fossil forms. It is necessary that I should say some words about this, because there are in this and other countries certain people who strongly urge the amalgamation of these collections, coming out with such absurd *dicta* as that one specimen should not be separated from another because it happens to be preserved in stone instead of in spirits, maintaining that the evolution of life and the relations of the present to the past are far more easily seen if one has not to walk several hundred yards to see the living ally of a fossil species. They also believe that the zoologists are led into errors through their ignorance of extinct animals, an ignorance largely fostered by the museum custom of keeping them apart; and they deny that the paleontologist can properly understand the fossils with which he deals so long as he is prevented by the assumed necessities of museum arrangement from studying living forms *pari passu*. An intimate friend of my own, who happens to be officially connected with one of our greatest scientific establishments, has privately complained to me that his studies found yet another difficulty in the fact that the books which are supposed to deal with modern life are placed in two or three separate rooms at a considerable distance both from one another, and from the room that contains the books dealing with extinct life. Nor is this all. He adds that, when the necessities of the case compel him, as they often do, to visit one of the other libraries, he is actually scowled at as an intruder by his fellow-workers in that department. It is clear that in the institution to which my friend has the honor to belong the true museum-spirit is still flourishing with vigor. It is this

spirit, this idea of separation, of privacy, and, as it were, personal property, to which the Greeks appropriately applied the term ἰδίωσις—it is this that we curators must continue to foster, if we are seriously desirous of retarding science.

To carry on the ἰδέα ἰδιωτικά: a Museum should keep itself to itself; it has nothing to do with the Free Library, with the University, or with the Zoological Gardens. Do you wish to be overpowered by a lot of rowdy students coming and pawing over your specimens; or do you, as a peace-loving curator, wish to be dragged off to give an opinion upon some new accession of an animal that is possibly dangerous? Remember, too, that by this cooperation your collections are likely to be increased to an unmanageable extent and your hours of labor will be lengthened without a corresponding rise in salary.

This leads me to consider an exceedingly difficult question—the lending-out of specimens. It is, as you are aware, the rule of the British Museum never to let a single specimen that has once been registered pass outside its walls, except as a donation or an exchange. Other museums are either, as you may prefer to term it, less careful or less miserly. There can be no doubt that science is greatly advanced when a reliable investigator, working in one locality, is able to borrow from the museums of other cities or of other countries specimens that will aid his labors. On the other hand, there is this to be said in favor of the proceeding: that in a large number of cases the specimens that are thus loaned never return to the museum, and ultimately are lost to science. It therefore does not very much matter, so long as, if you lend them, you conveniently forget whither they have been sent, and so long as, if you keep them, you place the necessary obstacles in the way of the investigator.

But it may be retorted to the last argument: there is another way whereby these difficulties are avoided and science greatly advanced. *Videlicet*, one museum can exchange type-specimens or special collections with another. Such a solution of the problem was laid before us at Dublin by Dr. H. O. Forbes. Now, on this question of the dispersal of types, a conversation that I had with a leading English entomologist impressed me forcibly. New species of insects, he said, are being described at the rate of about *6,000 per annum*. Those who attempt to coordinate the scattered descriptions cannot possibly do so without comparing the type-specimens. Experience shows it to be impossible for even an expert to draw up a description that shall be accepted as recognizable by another expert. Further, no entomologist of ordinary human powers can retain in his memory the conception of any one species, much less of three or four hundred, sufficiently well for him to compare specimens in one museum with those in another, unless he can set them side by side. For any real advance in this subject, the type-specimens of all the species of a family must be gathered together in one room, so that the specialist may examine and compare them directly. This could be done, either by the various type-specimens being lent for some time to another museum, or by a permanent interchange of specimens—one museum specializing in Hymenoptera, another in Diptera, and so on. The difficulties are felt most strongly in entomology, but they affect ornithology, botany, conchology, and other branches of systematic biology to a marked extent. Obviously, then, we have it in our power to retard the advance of these sciences, or even to check it altogether, by jealously guarding our treasures, either forbidding them to leave their abodes under any circumstances whatever, or cleaving to our type-specimens as to some musty but sacred heirloom, useful only to aliens, but

a tattered badge of pride to ourselves. Here is a weapon, the use of which has far-reaching results that appeal to the imagination with the certain annihilation they inflict. Fellow-curators, grasp your weapon, and, more powerful than Canute, force back the advancing tide!

F. A. BATHER.

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INTERNAL SECRETIONS CONSIDERED IN RELATION TO VARIATION AND DEVELOPMENT.

THE so-called internal secretions of glands and other organs consist of products manufactured by them and passed back to the blood. Many of these products are known to be of very great importance to the adult organism; it is possible that they may be of no less importance to the developing organism and that we may here find a clue to some of the unsolved problems of development. Internal secretions have probably been longest recognized in case of the reproductive organs. The effects of castration, of non-development or development of these organs are well known. It has also been generally recognized that the influence of these organs depends on substances formed by them and given to the blood. What these substances are is still unknown, but there can be little doubt that their presence determines the development of other organs and characters, the so-called secondary sexual characters. The long recognized healing effect of removal of the ovary in women suffering from soft bones and the subsequent growth of bone and fatty tissue has been the subject of researches by Curatulo and Tarulli.* These authors concluded that the ovaries produce a substance which oxydizes the organic phosphorus compounds and thus cause their rapid destruction. The removal of the ovaries would seem to remove the destroying substance and hence to cause a deposition of phosphorus, and experiment showed that after extirpation of the ovary the excretion of phosphorus fell off one-half.

Perhaps the best known internal secretion is glycogen. This substance, made by the liver and given to the blood, is used as food by many other organs. Thanks to Hedin, Minkowsky and others, the internal secretion of the pancreas is now known to be a necessity to the organism, for if this organ is extirpated, the animal (mammal) quickly dies from diabetes mellitus. What the active substance is and whether it acts directly on the liver or through the nervous system is not yet decided. Equally important internal secretions are produced by the thyroid and thymus glands and the suprarenal capsules, the complete extirpation of any of which leads to rapid death, though life may be prolonged for a longer or shorter time by feeding the animal with the missing organ or injecting its extract into the system. Very striking is the effect of non-development or over-development of the thyroid on the cranium. The low broad skull of the cretin forms a distinct type, and the rapid change in physiognomy in patients suffering from goitre after the reduction of the thyroid or the injection of thyroidin is well known. According to Brown-Séquard, the fatal results of extirpation of the kidney are due not to poisoning by urea, but to the lack of an internal kidney-secretion essential in some way to the organism. There can be no doubt that the muscles also form such a secretion, for it has been shown that the excitation of the breathing center on muscular activity is the consequence of some chemical substance given by the muscle to the blood. Perhaps a similar secretion is the ammonia manufactured by the mucous membrane of the stomach and carried to the liver, there to be elaborated into other products. Although such substances have not yet been

*Phys. Cent. IX.

isolated from the brain, salivary glands and some other organs, there seems good reason to believe that even these furnish to the blood substances peculiar to them.

It is, therefore, highly probable that all organs have besides their obvious function, a hidden function, in the maintenance, by means of their internal secretions, of the metabolic equilibrium of the body. Further, many of these secretions are absolutely essential to the life of other organs, and in certain cases, as in the thyroid and reproductive organs, they are necessary to the development of organs apparently not in any way connected with them. There can be little doubt that one of the prime uses of the blood is as a distributing agent of these substances, and that its coördinative function is one of its most important offies.

We are thus led to a possible explanation, along these lines, of the organic unity of organisms unprovided with a nervous system. It is highly probable that the internal secretions play an important rôle in the correlation of parts in the higher organisms. It is possible that this rôle becomes the principal one in case of the developing embryo or of organisms like the plants which have no nervous system. The internal secretions are also of interest in their bearing on the correlation of variations.

If an organ in one part of the organism depends in any manner upon the internal secretion of some other organ we may understand how the increased developement of the one may lead to an increased development of the other, though apparently in no way connected with it. Thus we could see how variations have arisen and how they have been perpetuated until they are themselves useful. Many organs, the beginnings of which could hardly have been useful enough to be acted upon by natural selection, may have been developed because they are correlated by means of their internal secretions with other organs which are useful. It would also be clear why certain organs or groups of organs vary together. If such organs are mutually interdependent in the manner indicated, then the diminution of one necessarily means the diminution of another and another and so on. In certain cases the diminution of one may lead to the growth of another organ. This is, perhaps, most strikingly seen in the case of castrated cattle, which are proverbially large boned and fat, the growth of the bone being correlated with the diminution of the internal secretion of the sexual organs. In another case, where two organs were dependent on the internal secretion of some third, the suppression of one of the two might lead to a compensatory growth in the other.

That the internal secretions play a part in embryonic differentiations seems very probable. Striking examples of their importance in the later stages of development are afforded by the thyroid and the reproductive glands, already referred to. Lack of development of the thyroid hinders the development of the cranium and the whole body. If cretins be fed on thyroid they increase both in size and intelligence. The development of the sexual organs is essential to that of many other so-called secondary sexual characters. The same may very well be the case in the embryo. Thus an organ called into being by a previous organ may in its turn determine, through an ininternal secretion, the development of a succeeding organ ; and we should here have an explanation of the persistence of rudiments, or the temporary appearance of glands and organs which later disappear and seem to fulfill no function whatever. They may be necessary to the organism through their internal secretions, which give the necessary stimuli to the development of other organs which are permanent.*

*See note at end.

I would suggest also that the internal secretions may possibly give the explanation of the modifying influence of the male element on the surrounding mother-tissue forming the fruit in plants. Darwin notes many cases of hybrids in which the fruit, though composed of purely maternal tissue, nevertheless plainly shows paternal characters. He explained these cases by the wandering of the pangens. It is not impossible that his 'pangens,' not only here, but in other cases, may be nothing else than the internal secretions. There can be little doubt, furthermore, that the internal secretions from the fœtus play a very considerable part in the modification of the maternal organism during pregnancy.

The foregoing suggestions are difficult of proof, but they do not seem to me inherently improbable, since it is altogether unlikely that the metabolic coordination, which certainly exists in the adult organism, comes into being only after the close of embryonic development, and only in such organisms as possess a well developed vascular system. It is well, too, to bear these internal secretions in mind in the study of the development of organisms. Such an organ as the shell-gland of the molluscs may be of vastly greater value to the organism as a manufacturer of an internal secretion than as the maker of a protective shell.

Albert Mathews,

Former Fellow in Biology, Columbia University.

Marburg, Germany.

[Note: The above interesting suggestion regarding the physiological *rôle* of internal secretions in development is, as far as I know, new. It is obvious, however, that the interpretation given of rudimentary or temporary organs in development is nearly related to that of Kleinenberg, with which the author is apparently unacquainted. Kleinenberg long since held that the permanent parts of the embryo might appear and be guided in their development 'through the stimulus or by the aid' of 'rudimentary' as well as of obviously functional organs; and that 'when these (the permanent organs) have attained a certain degree of independence the intermediary organ, having played its part, may be placed on the retired list' (*Lopadorhynchus*, 1886, p. 223). Mr. Mathews' suggestion has the great merit of supplying an intelligible working hypothesis regarding the nature of the 'stimulus' or the 'aid' given by the intermediary organ, and it seems well worthy the attention of experimental embryologists.

E. B. W.]

A LAYMAN'S VIEWS ON SPECIFIC NOMENCLATURE.

Anything that Dr. Hart Merriam writes is sure to be of great value. He is one of the leading mammalogists and he has laid all men interested in biology under a heavy debt by reviving the best traditions of the old-school faunal naturalist and showing that among the students of the science of life there is room for other men in addition to the section cutter, the microscopist and the histologist. There are a good many of us who look forward to the publication of his great work on the North American Mammals, including their life histories, as to something which will mark a real epoch in scientific work on this continent.

Having made this kind of preface, everyone will naturally and rightly conclude that I intend to say something in dissent from some of Dr. Merriam's views. I have just been reading his very interesting pamphlet on the smaller North American wolves, commonly called prairie wolves, or coyotes. His facts and deductions are most important; he has shown for the first time how many different races of coyotes there are, together with their inter-relationships and

their distribution in groups which coincide with the geographical divisions of their habitat. For the way in which he has worked out this, the most important, part of the article, no one can feel anything but admiration. But I quarrel with the terminology by which he seeks to describe the results at which he has arrived. He divides the coyote into a large number of different species, giving to each full specific rank and a specific name, in accordance with the theory of binomial nomenclature.

Now, terminology is a matter of mere convenience, and it is nothing like as important as the facts themselves. Nevertheless terminology has a certain importance of its own. It is especially important that it should not be clumsy or such as to confuse or mislead the student. Although species is a less arbitrary term than genus, still it remains true that it is more or less arbitrary. If one man chooses to consider as species what other men generally agree in treating merely as varieties it is unfortunate, both because the word is twisted away from its common use and further because it confuses matters to use it in a new sense to the exclusion of the word commonly used in that sense. Moreover, it is a pity where it can be avoided, to use the word so that it has entirely different weights in different cases.

I can illustrate what I mean by reference to the terminology used in describing the geographical distribution of mammals. It is not very important whether we call the great primary division of the world, faunistically considered, realms or regions. But it is important that we should not use the words first in one sense and then in another, and above all that we should not use the same word with totally different values. For example, Mr. Wallace's classification was absurd in so far as he made the Nearctic, Palearctic, Neotropical and Australian regions of equal value. There are differences between the mammalian faunas of northern North America and northern Eurasia, but they are utterly trivial as compared with the differences which divide the fauna of both regions from the fauna of either South America or Australia, or indeed of South Africa. To indicate by the nomenclature used that the differences are of equal importance in the four cases is as misleading as it would be to describe the ethnology of the United States in terms that would imply that the New Englanders, the Kentuckians, the Indians and the Negroes formed four divisions of about even rank. There are differences between the New Englanders and the Kentuckians; but no one would dream of distinguishing the two by terms that would imply that they were as widely separated as either is from the Indians or Negroes.

It seems to me that the same principle should hold true of the excessive multiplication of specific terms to describe the different varieties of a group of animals like the coyote. Specific as well as generic terms are quite as useful in denoting likeness as in denoting unlikeness. The excessive multiplication of the species in the books cannot, as it seems to me, serve any useful purpose, and may eventually destroy all the good of the Latin binomial nomenclature. In the group of wolves, for instance, so far as North America is concerned, the really important points to remember and to bring out are that there are two types: one, the small wolf, the coyote, which, wherever found, is sharply separated from the other, and only exists in a portion of North America; and the other, the large wolf, which is much more widely distributed over North America than the coyote, and is practically identical with the wolf of Europe and north Asia. There are a great many varieties of each, just as there are doubtless a great many varieties of wolves in Europe and north Asia. Among coyotes

it is an interesting fact that the coyote of the Little Missouri is bleached compared to the coyote of the upper Mississippi, and that he has larger teeth than the coyote of the Rio Grande; but it seems to me to be unwise to separate all these forms by giving them rank that would imply that they differ from one another as much as they differ from the great gray wolves of the same region. I understand perfectly that this is not what Dr. Merriam means, and that he would subdivide the genus into various groups so as to show that the species are not of equal value. Nevertheless, the fact remains that the important point is the essential likeness of all the coyotes one to the other, and their essential difference from the big wolves with which they are associated, and which are themselves essentially like the big wolves of Europe and north Asia; and it seems to me that these facts can best be brought out by including the coyote and the wolf in one genus and treating each as a species. Then the geographical and other varieties may or may or may not be treated as worthy of subspecific rank according to the exigencies of the particular case. The alternative is to use terms of super-specific value, including groups of minutely separated species; and this would be clumsy and would hardly seem worth while.

I will illustrate what I mean by referring to some other mammals. The points of resemblance between beasts like the wolverines, the beavers and the moose of the two northern continents are far more important than the points of difference. In each of these cases it does not matter much whether these animals are given separate, specific rank, because in each case the Old World and the New World representatives make up the whole genus; but even here it would seem to be a mistake to separate them specifically unless they are distinguished by characters of more than trivial weight. The wapiti and Scotch red deer, for instance, are markedly different, and the differences are so great that they should be expressed by the use of specific terms. If the American moose and Scandanavian elk are distinguished by specific terms of the same value, then it ought to mean that there is something like the same difference between them that there is between the red deer and the wapiti, and as far as our present knowledge goes this is not so. The wolverines, beavers and moose of the two continents should only be separated by specific terms, if the differences between each couple are of some weight, if they approximate the differences which divide the red deer and the wapiti, for instance—and I know that even these two may intergrade.

I would not dogmatically assert that even though forms intergrade they should not be sometimes separated by specific titles. In their extreme forms the grizzly bear and the little black bear are certainly utterly different, and I have shot these extreme forms within a mile of one another on the Big Horn Mountains. Whether they intergrade or not, there should be a sharp line of difference drawn between the typical representatives of these two kinds of bears; but I confess that I think that many of the multitude of 'species' of holarctic bears will have to be reduced to less than specific rank before we get a very clear idea of the true relationship of the bears of North America and northern Eurasia. The excessive multiplication of species based on trivial points of difference merely serves to obscure the groupings which are based on differences of real weight. Moreover, it has always seemed to me unwise to make the word species depend solely upon the accident of the survival or non-survival of some connecting link. Two closely connected forms may not intergrade, while two widely separated forms may; and it seems to me the term species should express the

fact of a wide and essential variation rather than the accident of the existence of a connecting link.

One more example and I am done. The cougar, or puma, is a perfectly distinct and well marked kind of cat, noteworthy not only for the sharpness with which its color and other points differentiate it from its spotted relatives, but also for the extent of its range. It seems to me it would be unwise because of any trivial differences to establish various species of cougars, separating the different races by terms of the same weight by which we separate, for instance, any one of them from the totally different jaguar. Here again the essential point is the likeness the cougars bear to one another, and their wide unlikeness to the great spotted cats. The Latin name we give them should indicate, by the employment of the generic term, their resemblance to all other cats, and by the employment of the specific term their fundamental agreement among themselves on points wherein they differ from all other cats. Of course, it would be possible to make the pumas into one genus, with another for the leopards, another for the lions, etc., etc.; but this again seems to me to be clumsy and, on the whole, misleading.

I quite realize that there is a certain amount of presumption in a layman criticising any conclusion reached by a trained scientific expert of the standing of Dr. Merriam. It must be remembered that my criticism is directed only to the expediency of the terminology by which he expresses certain of his results, and not in the least to the results themselves; in fact, it is because I am so ardent an admirer of Dr. Merriam's work that I wish to see it made, without any sacrifice of accuracy, so comprehensible in its terms as to be easily understood by the lay mind.

THEODORE ROOSEVELT.

CURRENT NOTES ON ANTHROPOLOGY.

CONTRIBUTIONS TO ETHNO-BOTANY.

IN the last number of the *Internat. Archiv für Ethnographie*, the editor, Dr. Schmeltz, reviews the progress of ethno-botany, referring with special emphasis to Professor Guppy's 'plant names of Polynesia' (published by the Victoria Institute, 1895). Such studies cast a light upon the early migration of tribes which cannot be obtained from other sources.

An interesting example is given in the *American Anthropologist*, February, by Mr. Walter Hough. It is upon 'The Hopi in relation to their Plant Environment.' How important their floral world, sparse as it is, has been to this people may be judged from the author's remark: "There is almost no plant which the Hopi do not use in some way, and there is none to which they have not given a name." An ample list is added, including the native name, the botanical title and the use to which the plant is put.

CANNIBALISM IN EUROPE.

WE rarely reflect how near in time modern civilization is to savagery. Less than a thousand years ago the Picts of Great Britain were man-eating barbarians. The recent researches of Matiegka, in Bohemia, prove that anthropophagy prevailed there in the bronze age (*Centralblatt für Anthropologie*, January, 1897). If we can trust mediæval authorities quoted by Dr. Krauss in the *Der Urquell*, B. I., they held distinctly in memory the period when the Wends and Slavs 'killed, cooked and ate' their aged relatives.

But this is quite surpassed by the evidence addicted by the same writer that the southern Slavonians even down to well within the present century were familiar with the custom of ceremonially eating the flesh of their enemies. Indeed, one of their songs, as late as 1820, refers to it as a recognized procedure. To taste the broth

made with the head of some famous warrior was believed to confer on women the possibility of similarly heroic offspring!

THE PRE-HISTORY OF NORTHERN EUROPE.

MAN first entered northern Europe in the Neolithic period; but that period, for that locality, is divided into an older epoch, when flint implements were not polished, and a later, when they were polished. The first of these was the age of the oldest Danish kitchen-middens; the oak was abundant there and in Scandinavia; but the men of the time did not carry on agriculture. The climate was warmer than it had been since. This epoch closed about 3000 B. C.

About that time the cultivation of barley and wheat was introduced, polished flint implements were manufactured, the beech began to abound, and the later refuse heaps and the dolmens were constructed. The distribution of this early culture indicates that it approached the north of Europe from the Iberian peninsula and probably from North Africa.

Such are the conclusions reached by Dr. E. H. L. Krause, in *Globus*, Bd. LXXI., No. 9, from the works of Andersson, Montelius and Meitzen.

D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

SCIENTIFIC NOTES AND NEWS.

HOW FLOWERS ATTRACT INSECTS.

PROFESSOR FÉLIX PLATEAU, of Ghent, has been making further careful experiments in the open air to determine what part the corolla and other conspicuous parts of the inflorescence of flowers bear in attracting insects, and has reached some results strikingly at variance with generally received opinions.

His first series of experiments (see SCIENCE, N. S., III., 474) were made on composite flowers with radiate inflorescence and resulted in the conclusion that their form and color play no part in attracting insects, these being guided by some other sense than sight—probably by odor.

In a second series he mutilated flowers of Lobelia, Œnothera, Ipomaea, Delphinum, Centaurea, Digitalis and Antirrhinum, with a wholly similar result, viz, to show that the colored organs of these flowers play a very unimportant rôle.

Further experiments, related in a third paper, lead him to make the following statements as their conclusion:

1. That insects show the most complete indifference for the different colors which flowers of the same species or of the same genus may present.

2. That they fly unhesitatingly toward flowers habitually neglected by them on account of their total lack or small supply of nectar, the moment one places in them an artificial nectar, represented by honey.

3. That they cease their visits to flowers from which the nectiferous portions have been eliminated (but in which the inflorescence remains intact) and that they renew their visits if one afterward replaces the eliminated nectar by honey.

The details of these experiments and observations are given with the utmost care and their importance cannot be questioned. The results are published in the *Bulletin* of the Belgian Academy.

SCIENTIFIC EXHIBITS OF THE GOVERNMENT AT THE TENNESSEE EXPOSITION.

DR. W. F. MORSELL writes that the government scientific exhibits for the Tennessee Exposition, which opens on May 1st, are well advanced. Exhibits will be made by the National Museum, taken from its numerous departments, and the Smithsonian Institution will include in the complete set of its publications the book prepared in celebration of its semi-centennial. The Bureau of International Exchanges will show the extent of its work, and astronomical photographs will be sent from the Astrophysical Observatory. The Zoological Park will send a model of the Park about seven feet square, and the Bureau of American Ethnology will present a Kiowa camping circle.

The exhibit of the United States Geological

Survey will embrace two cases of minerals and a case of fossils. It will also include a suite of the rocks of the Educational Series. This is one of a number of duplicate suites, each consisting of 156 typical rocks, which the Geological Survey has been preparing for a number of years, to be distributed to universities and colleges for purposes of instruction. In addition to the above, the Survey will show twelve or fifteen relief models, most of them very fine, and a large collection of the topographic maps and geologic folios, as well as a number of transparencies and pictures of various kinds.

The exhibit of the Department of Agriculture is designed to show as completely as possible the character of the scientific work which this Department is doing in developing the agricultural resources of the country. Each one of the scientific bureas and divisions will have its allotted space, with characteristic exhibits of its peculiar functions. The Weather Bureau will show a complete set of the instruments used in observing the weather, with its maps, charts, etc.; the Bureau of Animal Industry, typical specimens of animal parasites and illustrations of animal diseases; the Forestry Division, the forest resources of the country, particularly of the South; the Division of Entomology, the insects most injurious to Southern crops, with wax models of the corn and the cotton plant, insecticide apparatus, etc. The office of Fibre Investigations will show specimens of economic fibres (hemp, flax, etc.); the Division of Pomology, wax models of native fruits; the Division of Vegetable Pathology, specimens illustrating typical plant diseases; the Division of Biological Survey, the birds and wild mammals of the country, and the Division of Botany, useful and harmful plants. This Division will show also specimens of the various useful seeds, and will illustrate the methods employed in the Department's seed-testing laboratory. The space allotted to the Division of Agrostology will be devoted to a display of the grasses used for forage, for binding sandy soils, etc.

GENERAL.

PROFESSOR HENRY SIDGWICK, professor of ethics in Cambridge University, has been elected a member of the Danish Royal Society of Sciences; Professor Rudolf Heidenhain, professor of physiology at Breslau, a member of the Royal Society of London, and Dr. Salenski, sometime professor of zoology in the University of Odessa, a member of the St. Petersburg Academy of Sciences.

THE Honorary Medal of the Royal College of Surgeons was presented to Lord Lister and Sir James Paget at the last meeting of the Council of the College. This medal has been conferred but eight times previously during the present century—on the last occasion on Sir Richard Owen.

PROFESSOR J. MARK BALDWIN, of Princeton, has been awarded the gold medal offered by the Royal Academy of Science and Letters of Denmark for the best work on a general question in social ethics.

DR. KARL BOHLIN, of Upsala, Sweden, has has been appointed Astronomer to the Royal Academy of Sciences and Director of the Observatory at Stockholm.

AT a meeting of the Royal College of Physicians, of London, on April 12th, Mr. Samuel Wilks, M.D., F.R.S., was re-elected President of the College. A portrait, by Sir Thomas Lawrence, of Sir Henry Halford, President of the College from 1820 to 1844, was received from the executors of his grandson, the late Sir Henry St. John Halford, who had bequeathed it to the College.

AT the public exercises held on February 22d in honor of the twenty-first anniversary of the Johns Hopkins University, Professor Welch, on behalf of the friends and associates of Professor Newcomb, asked that he sit for a portrait to be given to the University. The remarks of Professor Welch as reported in the University *Circular* were as follows: "The custom which prevails in many foreign universities of celebrating, by some memorial, epochs in the lives of distinguished teachers and investigators connected with the university is one which can only be commended. A similar custom is finding increasing favor within recent years in this country, where so few material honors attend success in university and scientific careers. The colleagues and other friends of Professor

Newcomb desire to manifest their affectionate regard and their high appreciation of his services to science and to this University, and to mark an epoch in his life, by asking him to sit for a portrait to be painted in oil and presented to the Johns Hopkins University. It is just forty years since he left the work of a school teacher in the State of Maryland to engage in the mathematical service of the United States government. It is twenty years since he became senior professor of mathematics in the United States navy and editor of the *American Ephemeris and Nautical Almanac*. For many years he held the post of Astronomer in the Naval Observatory at Washington. With the Johns Hopkins University he has been closely associated since its foundation. He has been honored in unusual degree by academic distinctions and by election to membership in learned societies both in this country and Europe. His numerous contributions to science have received the highest possible recognition. This is not the occasion, nor am I the one, to attempt to estimate, in detail, the significance and the value of these contributions. The judgment of one's own peers is the test of the worth of discoveries in pure science. The great mathematician, Professor Cayley, has pointed out the rare combination, in Professor Newcomb's publications, of mathematical skill and power and of good, hard work devoted to the furtherance of mathematical science. When the blue ribbon of science, the Copley medal, was conferred upon our colleague by the Royal Society of London, attention was publicly called to the fact that he had won his distinction especially by his contributions to the science of gravitation and that his name was worthy to be remembered in the domain forever associated with the illustrious Isaac Newton. Professor Newcomb, your friends and colleagues now ask permission to place your portrait by the side of that of your colleague, Professor Gildersleeve, that thus there may be here silent and enduring tokens of the honor which this University bestows upon the man of letters and the man of science." Professor Newcomb responded briefly, acceding to this request.

WE recently expressed the hope that the valuable physiological library of the late Professor Du Bois-Reymond might be secured for an American institution. We now learn that Dr. Nicholas Senn has bought the library and has presented it to the Newberry Library of Chicago.

IT is stated that the widow of the late Professor Charcot has resigned the annual pension of 2,000 francs which she received from the State, in favor of other widows and children of professors or *Agrégés* of the Faculty of Medicine of Paris who have died without leaving provision for their survivors.

PRINCETON UNIVERSITY will send its fourteenth geological expedition to the West during the coming summer. The party will be under the direction of Professor Scott and will make paleontological and geological studies and collections in South Dakota.

PROFESSOR LAWRENCE BRUNER, of the University of Nebraska, sailed April 27th for Buenos Ayres, where he will spend a year investigating the injurious locusts which have recently increased enormously in three of the eastern provinces of the Argentine Republic. Professor Bruner goes out under the employment of a commission of business men and bankers of Buenos Ayres, who have raised a large sum of money for the purpose of fighting the locusts, and who, very wisely, decided that the first step should be to engage an expert of great experience and acknowledged reputation. The commission applied to the United States Minister, Mr. W. I. Buchanan, and Mr. Buchanan wrote at once to Major H. E. Alvord, of the United States Department of Agriculture, asking him to consult with several of the directors of the agricultural experiment stations in the United States and to select the best-fitted person for the work. The committee at once chose Professor Bruner, who has secured a year's leave of absence from the University of Nebraska. No better choice could possibly have been made. Professor Bruner was connected with the U. S. Entomological Commission in its thorough investigations of the Rocky Mountain locust, or Colorado grasshopper, in 1876 to 1880, and has since become known as one of the foremost workers on the order of Orthoptera in the United States. He has de-

voted much time and attention to migratory species.

We regret to record the death of M. Lucien Biart, a French physician resident in Mexico, who made contributions to ethnology and natural history. The Paris Museum of Natural History contains botanical and ornithological collections made by him.

Mr. Otis E. Bullock died in New York from yellow fever on April 22d. He contracted this disease in Central America while on his way to make collections in natural history for the Frank Blake Webster Co., of Hyde Park.

President Mendenhall lectured before the National Geographical Society, Washington, on February 23d, his subject being 'Weighing the Earth.'

The Illinois Child Study Society will hold its third annual congress at Englewood, Chicago, from April 26th to May 1st. It will be presided over by Colonel Francis W. Parker, and addresses are expected from President G. Stanley Hall, Professor John Dewey, Professor William L. Bryan and other leaders in the movement for the scientific study of children.

The Canadian Electrical Association will meet at Niagara Falls, Ontario, on June 2d, 3d and 4th.

An Educational Museum will be opened at the State House, Boston, on May 1st. It will include the exhibits of the Massachusetts schools at the Columbian Exposition, together with the work of other schools, school appliances and a pedagogical library.

The *Astronomical Journal*, Cambridge, Mass., offers for sale several complete and partial sets of the *Journal*, founded by B. A. Gould in 1849, with an interruption from 1861 to 1885. The complete set is offered for $70, or without the first volume, which is very rare, for $55.

Dr. Walter Wenga will edit and A. Priber, Berlin, will publish a new journal, *Zeitschrift für Criminal-Anthropologie.*

A journal devoted to the applications of the X-rays to medicine and surgery, entitled *La radiographie*, has been established in Paris. It is edited by Dr. Paulin-Méry.

The Paris Municipal Council has voted $1,000 towards the cost of installation and maintenance of a skiagraphic laboratory at the Trousseau Hospital.

The French Chamber has allowed a sum of 297,000 francs for the payment of expenses incurred on account of defensive measures taken against the plague.

The Executive Council of the Massachusetts State Board of Trade, at a meeting on April 21st, passed the following resolution: "That the Board recognizes the great advantages which the general adoption of the metric system of weights and measures will promote, favors not only its general use and practice, but also endorses the bill now before the Congress of the United States which provides for the adoption of this system as the only system in the several departments of the United States government. And, further, that the Secretary of the Board inform the chairman of the Committee on Coinage, Weights and Measures of the vote of this Board."

President McKinley and Secretary Sherman have recommended that Congress make an appropriation of $350,000 for the representation of the United States at the Paris Exposition of 1900. Such provision will doubtless be made, and we hope that the example of Germany and other nations at the Chicago Exposition will be followed, and that scientific and educational matters will be well represented.

The Executive Committee of the Tennessee Centennial Exposition (which opens May 1st and continues six months) has petitioned the Board of Trustees of Vanderbilt University for the release of Dr. William L. Dudley, professor of chemistry, for the remainder of the present academic year, in order that they might engage his services. The University authorities granted the request, and the Centennial management has elected Dr. Dudley to the position of 'Director of Affairs,' giving him full charge of the executive management of the Exposition.

At a meeting of the Royal Botanical Society of London, on April 10th, the Secretary, Mr. J. B. Sowerby, gave an account of the cultivation and manufacture, into paper, of esparto grass, illustrating it by specimens and growing plants from the gardens. According to the report in the

London *Times*, the raw material is chiefly obtained from the north of Africa, Algiers being the center of distribution. The plant producing it, *Stipa tenacissima*, is capable of living under the most adverse conditions, being often found flourishing in the deserts in places where no other vegetable life can exist. It was suggested by the lecturer that this would be a most suitable plant by means of which the deserts of Sahara might not only be reclaimed, but turned into a source of profit. For many years past esparto grass has very largely superseded rags and similar substances in the manufacture of paper, and enormous quantities are annually imported to England for the purpose. Samples of paper made of esparto, in various stages of its manufacture, were shown by Mr. Layton, from the mills of Messrs. Weir, of Alloa, who consume over 7,000 tons of this material per annum.

SIR BENJAMIN STONE, M.P., has been in correspondence with the authorities of the British Museum on the subject of a proposal to establish a national photographic record collection. In a letter to the Board of Trustees of the British Museum, Sir Benjamin Stone offered for acceptance a series of 100 platinotype photographic views of Westminster Abbey, hoping that this would be the commencement of a national photographic record and survey collection to be under the direction and in charge of the British Museum authorities. In replying to Sir Benjamin Stone the Trustees state that they are in full agreement with him that such a record survey collection, if carefully and systematically brought together, cannot fail to be of the greatest value and interest both to the present and to future generations, and they are most willing to take charge of the photographs which from time to time may be deposited with them. It is proposed to form a preliminary committee to organize the work and to invite to act upon it, representatives of the Royal Society, the Society of Antiquaries, the Royal Photographic Society, the Royal Institute of British Architects, the Royal Archæological Institute, the Royal Geographical Society, the Trustees of the British Museum, and others. The Council of the Warwickshire Photographic Survey have promised a first contribution of 100 pictures of that county.

A COMMITTEE of the House of Commons, consisting of Sir E. Hamilton, Sir A. Godley and Mr. G. H. Murray has been appointed to inquire into the organization, pay and duties of the staff of the British Museum, including the system under which the staff is recruited and the reasons for or against competitive examination, either limited or otherwise; the classification, scale of salary, and hours of attendance required to insure the efficient and proper discharge of the duties of the establishment, and whether it is practicable to assign clerical and routine work wholly or in part to clerks of the second division; and, generally, any matters connected with the Museum establishment in regard to which they may be of opinion that alteration of existing regulations is desirable.

AN exhibition of agriculture and forestry will be held in Vienna by the Imperial and Royal Agricultural Society from May 7 to October 9, 1898. The following sections are intended to be of an international character: (1) Machinery and implements for agriculture and forestry. (2) Machinery and implements for agricultural industry. (3) Dairy machinery and appliances. (4) Fertilizers, feeding stuffs, and chemical products for agricultural and forest purposes. (5) Veterinary science. (6) Agricultural improvements, building and engineering. (7) Agricultural and forest education, research work, statistics and literature.

MR. C. T. HEYCOCK, F. R. S., lectured before the Royal Institution on April 8th on 'Metallic Alloys and the Theory of Solution.' According to the London *Times* the lecturer showed a number of experiments which established an analogy between the solution of a substance such as sugar in water and the solution of metals in each other. Just as the freezing point of a solution of a salt in water was lower than that of pure water, so the freezing point of a solution of a metal, such as thallium, in mercury was lower than that of pure mercury. After explaining that there was no essential difference between the two phenomena he stated that the remarkable theory of Van t'Hoff, to the effect that a substance in dilute solution existed within the liquid in a state resembling a gas, afforded the best clue to the interpreta-

tion of the results. This contention was supported by means of a table proving that the results arrived at by experiment agreed with those predicted by the theory. He showed that a weak solution of permanganate of potash when frozen yielded at first nothing but pure colorless ice, all the color, and hence all the salt in solution, becoming concentrated in the central unfrozen part. While seeking to establish that the same held true for the metals Mr. Neville and himself had hit on a method which he believed to be one of importance and which was shown that evening for the first time. Gold was very readily dissolved by metallic sodium, and if a solution of gold in sodium were allowed to solidify very slowly then sections cut from the solid alloy would appear perfectly uniform to the eye. If, however, the sections were placed on a photographic plate and exposed to the X-rays, on developing the plate a picture was obtained showing the actual structure of the solid alloy, the sodium being transparent to these rays, while the gold was opaque. By means of lantern slides sections were exhibited cut from sodium-gold alloys containing different percentages of gold. These sections showed that crystalline plates of sodium traversed the mass both horizontally and vertically, and that the gold, as the solution solidified, had become concentrated between the crystalline plates of sodium. The analogy between the solidification of an alloy and the solidification of an aqueous solution was thus established.

UNIVERSITY AND EDUCATIONAL NEWS.

THE will of the late Judge B. R. Shelden, of Rockford, Ill., bequeathes $100,000 to Williams College, $100,000 to the Hampton Institute and $10,000 to Rockford College.

IT is stated in the New York *Medical Record* that Dr. William H. Welch and Dr. William Osler, of the Johns Hopkins Medical School, of Baltimore, have declined the call extended to them by the University of New York, which has lately been consolidated with Bellevue Hospital.

PROFESSOR ALBERT BUSHNELL HART has been promoted to a full professorship of physics at Harvard University.

PROFESSOR W. F. EDWARDS has been elected President of the Washington University, Seattle, in the place of Dr. Mark W. Harrington.

DR. ANDR. LIPP has been appointed professor of analytical chemistry in the Polytechnic Institute at Munich. Professor Sissingle, of the Polytechnic Institute of Delft, has been called to the chair of physics in the University of Amsterdam, and Dr. George Scheffers, of Leipzig, to an assistant professorship of mathematics in the Polytechnic Institute in Darmstadt. Dr. Wülfing, docent in mineralogy at Tübingen, and Dr. Max Siegfried, docent in physiology at Leipzig, have been promoted to assistant professorships.

DISCUSSION AND CORRESPONDENCE.

THE RE-DISTRIBUTION OF TYPE-SPECIMENS IN MUSEUMS.

I CAN'T think why Mr. F. A. Lucas, in his most friendly review of my paper 'How may Museums best Retard the Advance of Science?' (SCIENCE, April 2, V., p. 543), should say: "Mr. Bather seems to use the term type a little vaguely, as one does not feel quite sure whether he means type or typical material." The term I used was 'type-specimen,' which has for me, and doubtless for Mr. Lucas, one meaning and one only. The question raised in my paper has been much discussed of late in England; permit me to put my view, which differs from that of Mr. Lucas, without satirical obscurity.

The object of museums is after all to advance and not to retard science. Take the case of a provincial museum, say at Thurso, in the extreme north of Scotland; suppose that this museum by some chance acquires a single specimen of a new Mexican beetle; suppose that some wandering 'Koleopterolog' from Germany chances on this and describes it in the *Zoologischer Anzeiger*. The specimen is now a type-specimen, "and no museum," says Mr. Lucas, "can afford to permanently part with these." But does the retention of this specimen at Thurso, in charge of some underpaid jack-of-all-trades curator, do anything

other than retard science? Would it not be better for all parties, including the museum and people of Thurso, if this priceless specimen were sent to Mexico, or to Washington, or to the Godman-Salvin collection in London, or even to Berlin, in exchange for a good teaching set of zoological specimens intelligible to the Thurso fisher-people?

This is a strong, though by no means an impossible case. Every specialist knows similar instances. Of what advantage was it to science that, when Dr. Otto Jaekel was writing his admirable memoir on the Devonian crinoids of Germany, all the type-specimens described by Schultze in his 'Echinodermen des Eifler Kalkes' were locked up in dusty boxes in a store room at Harvard? As things are, the type-specimens of any group of animals or plants, whether a zoological group, a geographical group, or a stratigraphical group, will be found by the specialist scattered all over the world without reference to country or to facilities for study. And we museum curators go on adding to this confusion as hard as ever we can, with the aid of preliminary notices, and stretch miserly hands over specimens that are wanted most in some center of research 8,000 miles away. We advance our museums, but we retard science.

And yet there are some of us who are also students and lovers of science. We wish to use our powers for her advancement. This we think might be done partly by the collection of the type-specimens of a single group in a single museum, partly by the restoration of type-specimens to the country of their origin, provided that it possessed a museum capable of preserving them unharmed, partly by the withdrawal of type-specimens from small local museums where they 'waste their sweetness, etc.,' and are far from safe, to the larger museums with permanent endowment. We do not wish any museum to suffer; exchange is no robbery, and in this case might be as much gain to each contracting party as it would be to scientific investigators.

Another small point in Mr. Lucas' notice provokes an explanation. "On the question of loaning specimens," says he, "Mr. Bather dwells lightly, owing to his connection with the British Museum, whose policy in this respect is well known." This is Mr. Lucas' reason, not mine. My view is that type-specimens should not be lent (they should, if necessary, be exchanged); but other material should be lent freely to responsible workers. There is always a danger of loss; but, while the lost type-specimen can never be replaced, the gain to the museum and to science through the study and description of ordinary specimens more than counterbalance the occasional loss of one. This is not the policy of the British Museum, and no remarks of mine are likely to make it so. Similarly my opinions will not prevent me from borrowing type-specimens of crinoids from any museum rash enough to lend them to me.

F. A. Bather.

British Museum (Nat. Hist.), April 15, 1897.

THE QUATERNARY OF MISSOURI.

To the Editor of Science: After reading the quite satisfactory review of my report on the Quaternary of Missouri, in your issue of April 9th, some unanswered questions were left in my mind. As the answers may be of interest to others I venture to offer them through your columns. Mr. Hershey suggests that the idea that the *loess* 'area deposited by broad semilacustrine stream floods,' 'would not have originated upon certain other areas, for instance, the upper Mississippi region.' Is not this virtually the origin conceived the most probable for the loess of the 'Driftless Area' by Chamberlin and Salisbury in the 6th Annual Report, U. S. Geol. Survey?

Mr. Hershey, if I understood rightly, suggests that the loess deposits of Missouri and of southern Illinois as well as of the upper Mississippi were formed in a vast lake or arm of the sea. If that be the case I would ask (1) why no traces of beach ridges have been preserved anywhere, and (2) how he would account for the absence of loess from surfaces along the Mississippi below the supposed 'barrier' much lower than the general level of the loess northwest of that 'barrier,' viz., the Osage Gasconade divide?

If I had been able to find beach ridges and been able to make the margin of the loess south of the Missouri river pass easily into that west

of the Mississippi I should have been only too ready to accept the lacustrine hypothesis.

J. E. TODD.

A 'DRIFTLESS' RIDGE.

TO THE EDITOR OF SCIENCE: In reviewing, in the April 9th number of your journal, Professor Todd's report on the quaternary geology of Missouri, I mentioned a certain 'driftless' ridge in Pike and Calhoun counties, in Illinois, and referred its study to Mr. Frank Leverett. My attention has been called to the fact that the driftless nature of this ridge was discovered by Professor R. D. Salisbury (see Proc. A. A. A. S., Washington meeting, 1891, pp. 251–253), and that its study was largely accomplished by him.

In reference to the sections of the old and new gorges of the Mississippi river, between Montrose and Keokuk, Iowa, I wish to add to what I have said previously, that they were published through the courtesy of the Iowa Geological Survey, to which institution their preparation should be credited.

O. H. HERSHEY.

SCIENTIFIC LITERATURE.

Diseases of Plants Induced by Cryptogamic Parasites. An Introduction to the Study of Pathogenic Fungi, Slime-Fungi, Bacteria and Algae. By DR. KARL FREIHERR VON TUBEUF. English edition by WILLIAM G. SMITH. Longmans, Green & Co., London, New York and Bombay. 1897.

The German edition of this work appeared in 1895 and was the first attempt at a comprehensive treatment of the diseases of plants caused by parasites of the class Thallophyta, chiefly parasitic fungi. Such a work has been long needed, but there have been many difficulties in the way of the successful preparation of it. The fact that many of the diseases were but little known, that the organism causing them had been but little studied, and that important contributions were constantly being made to our knowledge of these forms, made it exceedingly difficult to get a book of such dimensions through the press before important changes would be necessary in order that it should properly represent the then status of the subject. While the German edition when it appeared was welcomed because of the mass of information which was here for the first time brought together in a single book, it was notable for some important omissions, especially of work done in the United States. This was probably due in part to the fact that some of the investigations had not come to the notice of the author, and partly to a failure on his part during the press of the work to consult the American journals like the *Botanical Gazette* and the *Bulletin of the Torrey Botanical Club.* While it is evident there was no intent on the part of the author to ignore American work, the edition would have been more valuable had a little more time been given to investigations of this portion of the literature of the subject. Since, however, the work was intended primarily for the German-speaking people there is here some partial defence of the omissions.

The chief difficulty, however, that of keeping the work up to date while going through the press, was, from the very nature of the state of our knowledge of these subjects, an insurmountable one. This is forcibly illustrated in the fact that in the English edition, which appears within two years after the first edition, it was necessary to recast and rewrite the whole portion of the book which treats of the family Exoasceæ and the genus *Gymnosporangium*, so rapidly have investigations in these groups followed each other, and so greatly have the limitations of species been changed by a study of the physiological effects on the hosts on the one hand and of biological studies on the other.

In the preparation of the English edition the author, Dr. von Tubeuf, privatdocent in the University of Munich, has added much that was omitted from the first edition and has rewritten the sections already alluded to above. The English translator, William G. Smith, lecturer on plant physiology in the University of Edinburgh, has also assisted in enhancing the value of the work in some additions for which he alone is responsible. It is not often that an author is so fortunate in the selection of his translator as Dr. v. Tubeuf has been. Dr. Smith was at one time a pupil of the author in the laboratory of the University of Munich, and at the very time when the book was being prepared for the first edition, so that he was

familiar with its general plan and with the spirit of the author.

In looking over the bibliography, which includes the more important works consulted by the author and translator, it is interesting to note that the bulletins of the experiment stations in the United States have been given a place, and there are many references in the body of the work to the published investigations of several of these stations.

In defining the parasitism of the parasitic fungi, on account of the facultative nature of a large number of the species of both parasitic and saprophytic forms, the author believes that it is more correct to consider as parasites those which in their attack respond to the stimuli exerted upon them by living plant cells rather than as an adaptation to nutrition, being influenced in this respect by the researches of Pfeffer and Miyoshi. According to these investigations the stimulus seems to be a purely chemical one, and Miyoshi has shown that ordinary saprophytic fungi, as *Penicillium glaucum*, may be made to behave like a parasite by injecting a two per cent. solution of cane sugar into leaves.

The terminology applied to those forms which are not strictly obligate parasites or saprophytes is different from that employed by de Bary and others, the present author employing the terms 'hemi-parasites' and 'hemi-saprophytes.' The first chapter further deals with the mode of life of the parasitic fungi, their relation to the host and to its different tissues, and the various modifications of the mycelium into absorbent organs for the taking up of nutritive matters.

In the chapter on the reaction of the host to parasitic attack the work treats of the absorption of cell contents, the absorption of cells and tissues (notably in certain Ustilagineæ), the killing of host cells and tissues by ferments, the killing of organs or entire parts, the premature development of buds, preservation of the host plant, arrest of growth, atrophy, hypertrophy, and changes in cell contents of the host. Under the last head among other things is cited a kind of chlorosis produced by certain fungi on the host, when the green parts become bleached and lose their green color, as in the case of the attack of many of the Exoasceæ. This is termed 'mycetogenous chlorosis.' Contrasted with this are those cases which have probably been observed by all students of parasitic fungi, in which the affected portions of the leaves or shoots remain green longer, while the unaffected parts become pale and lose their green color. A third case is termed 'mycetogenous chloranthy,' that is the development of green color in the floral parts, as in the petals and stamens of *Brassica nigra* and *Sisymbrium pannonicum* attacked by *Cystopus* and *Peronospora*, and in the flowers of *Anemone ranunculoides* attacked by *Æcidium punctatum*. In some cases of hypertrophy the cell sap assumes a rose color on the sunny side, as in galls, caused by Exobasidium and in the bracts of the catkins of alder attacked by Exoascus. Carmine and yellow colors also occur, and yellow color may sometimes result from the yellow oil contents of the mycelium lying in the tissues. The accumulation of starch in parts of the host attacked by certain fungi is noted, as in the spruce needles when affected by *Lophodermium macrospermum* at a time when it is only being slowly formed in unaffected needles. Starch preservation is noted in oak wood destroyed by two fungi simultaneously. This chapter further deals with the effect of the mycelium in dissolving starch grains, wood cell walls and the effects of fungi on the anatomical structure of their hosts.

Under 'mutualism' or 'symbiosis' in the stricter sense the author first cites the much discussed case of the lichens. Here, the author claims, "as a result of the union of fungus and alga, a living organism originates, which in form necessities, and mode of life is quite new, and differs completely from either of its components." In dilating upon the evolution of this new organism the author compares it with water, which is the result of the combination of oxygen and hydrogen, or to a certain extent to the new individual, which is produced by the union of sexual cells. "These and other examples," he says, "will serve to illustrate how we have in the lichen an organism with peculiarities of structure and of life widely differing from those of either an alga or a fungus." This unification of two living beings into an individual whole the author terms 'individuation'

(individualismus). While there are a number of lichenologists at the present day who accept this theory of the lichen, which has been elaborated farther by Reinke, it should be understood that there are others who are not convinced by the 'relentless' logic which separates the lichen fungi as a distinct class, but who look upon this relation of fungus and alga as parasitism in which the fungus is no more dependent on the alga than are certain other fungi upon their hosts. In the perennial parasitic Exoasceæ, for example, the affected parts of the host in the case of such species as *Exoascus deformans*, *E. pruni*, etc., are totally unlike the normal parts of the host, and during their existence, in 'form, necessities and mode of life, differ completely from either of their components.' This deformed structure differs from the lichen only in the fact that the entire host is not a changed and 'new' being. But here there is no necessity for this, since the host is a bulky, multicellular structure, while the alga which is associated with the fungus is often an unicellular organism, or one of a few cells, or at most in a few cases a comparatively complex organism of small size, so that it could not afford a sufficient amount of nutrition for the fungus unless woven in close connection with the fungus threads.

The author recognizes that the same kind of 'individuation' which is manifested in the lichens also exists in the modified structure brought about by the parasitism of many of the fungi when he cites the negative geotropism characteristic of witches' brooms, since the new growth is no longer controlled by the same laws of growth as the normal lateral branches. Further, he points out that this structure possesses other characteristics not exhibited by normal plants when the witches' broom of the silver fir casts its needles each year. In other cases they bear no flowers or fruit. "From these facts it can be deduced that parts of plants attacked by fungi exhibit that kind of symbiosis with the fungus which we call individuation, the joint community behaving more or less as a parasite on the stem or branches of the host plant. This is clearly the case where the attacked parts exhibit increased growth, and at the same time a diminished production of chlorophyll resulting from degeneration of chloroplasts. Such parts of plants are quite as individualized as the lichens, with the single distinction that they remain in communication with the parent plant and draw nourishment from it."

From this it would seem reasonable to conclude that if the fungi which attack algæ are to be placed in a separate class because of this individualized condition, as some contend, these 'individualized' parts of vascular plants should be separated as another class of organisms. We do not understand, however, from his discussion that the author sanctions the separation of the lichen fungus from other fungi as a distinct class rather than on the ground of convenience. It has been a matter of convenience as well as one of taste to study and publish the lichens separately, just as it is often a matter of convenience to separate the parasitic fungi from others. But neither matters of convenience, nor taste, nor continued dependence upon some other organism and physiological amalgamation with it for limited periods, should be the ruling principle in natural taxonomy.

The word 'individuation' (individualismus) is misleading, unless the author means by it that the lichen has become a being with individual traits as distinct as those beings which are recognized as individuals. If this latter interpretation is given it would seem to violate one of the fundamental criteria of an individual being, namely, that in reproduction it must pass through the one-cell stage, while the lichen thallus is never originated by less than two cells.

The author uses the term nutricism to denote the 'symbiotic' relation of 'mycorhiza' to their hosts, as exampled in the case of Monotropa and the filamentous fungus covering its roots, the mycodomatia of the alder, the legumes, orchids, etc. These general topics make up the first part of the book and cover about 100 pages.

The second part covers over 400 pages and treats first of the pathogenic fungi and lastly of the pathogenic algæ.

The fungi are taken up in the following order: Chytridiaceæ, Zygomycetes, Oomycetes, Ascomycetes, Ustilagineæ, Uredineæ, Basidiomycetes. Then follow the 'Fungi Imperfecti.' The discussion of each genus is preceded by a

description of its principal characters. A few of the important species in each genus are quite fully described and in many cases illustrated. These are followed by a further enumeration of a number of other species with their hosts and localities, the species in many cases for Britain and the United States being indicated.

The book is very fully illustrated, a very large number of the illustrations being new, either from the pencil of the author or from excellent photographs. As foot notes, there are very copious references to works even in cases where space would not permit of a discussion of their contents.

Neither the author nor the translator pretends to completeness, but modestly offer excuses for faults which under the conditions could not be well avoided. These can well be overlooked in view of the great amount of information contained in the volume which will prove to be a very useful adjunct to reference works on parasitic fungi. When a new German edition shall be called for the author promises to thoroughly revise it and expresses the wish that those who have in the past sent him copies of their investigations continue to do so in order that he may make this edition as complete as possible.

GEO. F. ATKINSON.

CORNELL UNIVERSITY.

RECENT BOOKS ON QUATERNIONS.

1. *Theorie der Quaternionen.* VON DR. P. MOLENBROEK. Leiden, E. J. Brill. 1891. Pp. vii+284.
2. *Anwendung der Quaternionen auf die Geometrie.* By the same author. 1893. Pp. xv+257.
3. *The Outlines of Quaternions.* By. LIEUT.-COL. H. W. L. HIME. London, Longmans & Co. 1894. Pp. 190.
4. *A Primer of Quaternions.* By A. S. HATHAWAY. New York, Macmillan & Co. 1896. Pp. x+113.
5. *Utility of Quaternions in Physics.* By A. MCAULAY. Macmillan & Co. 1893. Pp. xiv+107.

The above books are all contributions to the literature of the Quaternion side of space-analysis. The first, by Dr. Molenbroek, is a carefully written exposition of Hamilton's theory; the author, if he does not examine the correspondence of the theory with exact science and established analysis, at least presents it so as to be internally consistent. For instance, he explains the fundamental rule $ij=k$ as meaning that a quadrant round the axis j followed by a quadrant round the axis i is equivalent to a quadrant round the axis k. Consistently with this, he explains the rule $i^2=-1$ as meaning that a quadrant round the axis i followed by a quadrant round the same axis is equivalent to a reversal. The treatise, however, does not go deep enough; for the subject of quaternion logarithms and exponentials is embraced in a 9-page appendix, and what is there given is the well-known theory of coplanar exponentials. It is only when diplanar exponentials are handled that problems can be attacked which are insoluble, or at least not readily solved by the ordinary methods of analysis. Dr. Molenbroek introduces an indefinite use of $\sqrt{-1}$ to signify a quadrant round some axis perpendicular to a given line. There are reasons for believing that in space-analysis $\sqrt{-1}$ is scalar in its nature, and that it distinguishes the hyperbolic angle from the circular angle. Anyhow, that is one definite meaning.

The third book, by Col. Hime, presents a very dim and imperfect outline, which it would be well for the beginner to avoid. By perusing it he may get his ideas confused, not only of analysis, but of mechanics; for example, at p. 33 the terms 'version,' 'torsion,' 'rotation,' 'twist,' are all used as synonymous. This is, at least, awkward, for one of the first things which a student of quaternions must do is to distinguish between the trigonometrical composition of angles and the mechanical composition of rotations. The author explains the rule $ij=k$ by saying that j and k each signify a unit vector, but i signifies a quadrantal versor which turns j into k. But he fails to observe that this explanation cannot apply to the complementary rule $i^2=-1$, for a quadrantal versor i operating on a unit vector i would leave it i. Chapter Tenth is devoted to the 'Interpretation of Quaternion Expressions;' thus for nine chapters the reader is supposed to be dealing with symbolical expressions. Would it not be

better if the real meaning of each expression were clear from the beginning?

The fourth book, by Professor Hathaway, presents a much better introduction to the method, and the student who masters it will find that he has acquired some real knowledge, not merely additional dexterity in formal manipulations. The exposition, as a matter of logic and of truth, is not all that can be desired, for it is based partly on formal laws, partly on mechanical truths. For example, the principle that the addition of vectors is associative is made to depend on an arbitrary definition of the equality of vectors, but the same principle for the product of quaternions is rested upon the composition of rotations of a rigid body.

The fifth book, by Mr. McAulay, has a different purpose from that of the others. It is an essay, not an introduction or a treatise, and the aim of the essay is to make good the following statements: First, that Quaternions are in such a stage of development as already to justify the practically complete banishment of Cartesian geometry from physical questions of a general nature; and second, that Quaternions will in physics produce many new results that cannot be produced by the rival and older theory. In the essay the author applies the quaternion analysis to the theories of elastic solids, electricity and magnetism and hydrodynamics. It is almost wholly a translation into quaternion notation of known results; the author has, however, endeavored to advance each of the theories mentioned in at least one direction.

It is evident that the utility of a method is best proved not by any essay, but by its extensive and fruitful use. How does it come about that the method of quaternions is so far from general and accepted use that it is still the subject of debate, misunderstanding and even ridicule? Not a few mathematicians agree with the opinion expressed by a German mathematician, that it is an aberration of the human intellect. The answer to the above question I believe to be as follows:

In the books before us, and, indeed, in all the works by members of the old school, it is admitted, even proclaimed, that the Hamiltonian analysis is a rival of the Cartesian analysis. Mr. McAulay talks of it as a new plant, independent of the old tree of analysis; and in their letter to SCIENCE proposing an international association Dr. Molenbroek and Mr. Kimura invited mathematicians to leave the old domain of Cartesian analysis. Now, when one who has been trained in the Cartesian analysis approaches the new method he finds that the notation is strange and the conventions contradictory of those to which he has been accustomed; consequently, he concludes, as David did about Saul's armor, that it is better in actual warfare to rely on a familiar weapon than on one which may be superior but is unproved.

What is the true relation of space-analysis to the Cartesian analysis? The quaternionist makes them rivals; there is the blunder. Space-analysis can be presented so as not to contradict or rival the Cartesian analysis, but, on the contrary, be consistent with and supplementary to it. The relation of the former to the latter is like that of algebra to arithmetic. Algebra is universal arithmetic; so space-analysis is universal Cartesian analysis; that is, it considers the properties of vectors which are independent of coordinates. Many theorems are readily proved by algebra which it would be difficult, if not impossible, to prove by arithmetic; similarly, many theorems can be readily proved by space-analysis which it is difficult, if not impossible, to prove by means of coordinates. If we wish numerical results, coordinates must be introduced, just as, if we wish numerical results, numbers must be introduced into the formula furnished by algebra.

Some writers express the opinion that agreement about notation is all that is required in order to render space-analysis generally accepted. But it appears to me that the difficulty is more deep-seated; the fundamental principles need to be discussed, and no notation can be adequate and lasting which is not built on the simplest and truest principles. I may mention briefly some points of principle which have to be settled.

It is unscientific to base the analysis partly on formal laws, partly on physical principles. By not distinguishing between simultaneous and successive addition Hamilton failed to discover the true generalization for space of the

exponential theorem. I have demonstrated that in space $e^p \times e^q = e^{p+q}$, and the demonstration shows conclusively that the Hamiltonian ideas about the addition of vectors require to be revised. Although I have asked quaternionists to point out any error in the demonstration, no error has been pointed out.

The Hamiltonian principle that a unit-vector may be identified with a quadrantal versor requires to be modified. The conception of a line does not involve the idea of an angle, whereas the conception of an angle involves the idea of two lines. The question reduces to the following: Can a line be conceived apart from an initial line? The answer appears to be yes, for Hamilton did not succeed in his endeavors to extend algebra to space until he abandoned the idea of an initial line and considered all three axes as equally real. The vector and the versor are complementary ideas, and just as a vector is expressed in terms of rectangular coordinates which are in their nature vectors, so a versor is expressed in terms of rectangular quadrantal coordinates which are in their nature versors.

On the other hand, a vector cannot take the place of the versor. To ignore the versor and more generally the quaternion is the mistake made by writers who confine space-analysis to vector-analysis, which is merely a branch. The very name vector-analysis implies a restricted view of space-analysis. The versor is the proper idea in spherical trigonometrical analysis, and in a modified form expresses the rotation of a rigid body. It leads up to higher ideas which express elliptic and hyperbolic angles and the motion of a body which is not rigid.

In mathematical analysis the product of two quantities having the same direction is positive, while that of two quantities having opposite directions is negative; consequently the square of a quantity is always positive. Consistent with this the reciprocal of a negative quantity is the negative of the reciprocal. Now, are all the quantities considered in algebra or the Cartesian analysis scalar quantities, or are they in some cases partial vectors? If in any case they are partial vectors (that is, component of a vector) then, in order to be consistent, the square of a vector in space must be positive and the reciprocal of a vector have the same direction as the vector.

The order of writing of the terms of a sum or the factors of a product should conform, as far as possible, to the order followed in mathematical analysis. There the natural order of writing is followed, from left to right, and, as in a determinant, from top to bottom. But in books on Quaternions, for example, Hathaway's Primer, p. 49, we have the Hebrew order of writing. This abnormal order of writing was adopted from the idea that a product of quaternions supposed an operand and that the operand ought to be on the right. As a matter of fact, in the expression for the rotation of a versor the operator is written both before and behind.

ALEXANDER MACFARLANE.

SCIENTIFIC JOURNALS.

JOURNAL OF GEOLOGY, APRIL–MAY.

PROFESSOR CHAMBERLIN continues his glacial studies in Greenland, giving a description of the Bowdoin glacier. This is a tongue of the great inland ice-cap which descends from the north into the head of Bowdoin Bay. On the west it is confluent with the Tuktoo and Sun glaciers. The Bowdoin glacier has a length of six or eight, and in its lower part a breadth of about two miles. It has a descent of 2,000 to 3,000 feet, and is notably crevassed. It discharges icebergs of considerable dimensions, the discharge varying greatly with the season. The west side does not present the usual vertical scarp, and this is thought to be due to the fact that the ground which should act as a reflecting plane is covered by protuberances from the Tuktoo glacier. The stratification and basal loading of the ice is much the same as in the glaciers previously described, though the débris does not rise so high. The bowlders were usually more rounded, and this rounding is of such a nature as to imply very considerable wear. This considerable rounding, the small amount of débris and its low position in the ice are especially significant in view of the fact that the Bowdoin is one of the larger tongues of the great icecap.

Dr. Henry Washington describes the Rocca Monfino region in the fourth of his Italian

Petrological Sketches. The rocks of the region belong to three periods of activity: (1) the leucitic characterized by leucites and leucite-tephrites, (2) the trachytic and (3) the basaltic. Among the rocks of the first period is a biotite-vulsinite, a rock intermediate between the trachytes and andesites. The silica is lower than in the vulsinites, the lime, iron and magnesia very much higher and the alkalies considerably lower. Chemically the rock is almost identical with ciminite, but in deference to the present mineralogical classification of rocks it is put with the vulsinites.

Are the Bowlder Clays of the Great Plains marine? is asked by Dr. George M. Dawson, and as a reason for the question he enumerates several species of foraminifera, in part modern forms, determined from the Canadian bowlder clays by Mr. Joseph Wright.

The Beauxite deposits of Arkansas are described by Professor John C. Branner. The beauxite deposits were discovered by the recent Geological Survey of that State and are of ferruginous, earthy and kaolin-like varieties with pisolitic structure. In age they probably belong to the Tertiary. They appear to have been laid down in water near the shore and, in part at least, to have been uncovered at low tide or broken up by storm waves, rolled, and finally left at or near where the material had originally lain. In the opinion of Professor Branner, before the eruptive syenites had cooled they were sunk beneath the Tertiary sea, and either by the contact of the sea water or the issuing of springs, whose waters had been in contact with the hot syenites, the aluminous materials were segregated as pisolite and sank near where they were formed. The beds have not been developed, though they could be used to advantage as a refractory material in the manufacture of iron and steel. The paper includes a considerable bibliography.

H. F. B.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES—SECTION OF GEOLOGY, APRIL 19, 1897.

THE evening of the monthly meeting of the Section was devoted to a reception, by the whole Academy, to Sir Archibald Geikie, Director-General of H. M. Geological Survey of Great Britain, who had just returned to this country for a brief visit after an absence of eighteen years. After an informal reception the meeting was called to order and addressed briefly by the President of the Academy, Professor J. J. Stevenson, who extended a most hearty welcome from the scientists of New York to the guest of the evening. Professor Stevenson was followed by Professor J. F. Kemp, the Chairman of the Section, who reviewed in a few words the greater contributions of Sir Archibald Geikie to the cause of geology. He spoke of his early work in Scotland, in France and in the western United States in the study of vulcanism, and paid particular attention to the work that had been done in Scotland on the metamorphic rocks. Professor Kemp concluded with a tribute to Sir Archibald as a naturalist, and spoke of the superior quality of work that is given the world by the man who is in love with nature and finds in the solitude of the wildness of nature his greatest company and inspiration.

The next speaker was the Secretary of the Section, who spoke particularly of the work of Sir Archibald Geikie as looked at from the standpoint of the teacher and physiographer. He reviewed hastily the character and quality of Geikie's Text-book and Class-book of Geology, and spoke more especially of the example this distinguished geologist has set in physiography in the masterly analysis of the physical features of Scotland given in his Scenery of Scotland.

The last address of welcome was given by Professor Angelo Heilprin, of Philadelphia, who spoke as a traveler and contrasted the knowledge of the geology of the world now with our knowledge at the time of Humboldt. He spoke of how much we owed to the guest we were welcoming for his work in bringing together the shreds of knowledge from all parts of the world and in building up a great mass of geological information, which is a vast help to all workers in geology and a stimulus to all.

In reply Sir Archibald Geikie expressed his thanks to the Academy for the very cordial reception that had been tendered him in New York. He contrasted the appearance of the

city eighteen years ago and now, and spoke of the great growth of New York vertically as well as horizontally. He paid a brief word of tribute to his friends of his former visit, particularly Newberry, Leidy, Dana, Cope and Hayden, whose help and good will have ever been a great inspiration to him.

In reviewing the work of world-wide reputation that the American geologists are producing, Sir Archibald Geikie paid a warm tribute to their industry, their perseverance, their breadth and their scientific acuteness. He contrasted, in a very favorable way to the United States, the policy of the British and United States governments in regard to the printing, publishing and distribution of government reports.

After these brief addresses an opportunity was given for meeting the guest of the evening, for personal social meetings among the members of the Academy, and for greeting the guests from a distance, including several well-known geologists.

RICHARD E. DODGE,
Secretary.

BOSTON SOCIETY OF NATURAL HISTORY.

THE Society met February 17th; seventy-three persons present.

Professor N. S. Shaler spoke of the subterranean water of southeastern New England, stating briefly the distribution of earth water, the characters of the superficial deposits, and of the supply yielded by the deeper rocks. In America the bed rocks yield but little water, a case of supply being unknown. Good water depends upon the length of time the rocks have had to decay; it is obtained from the uncompacted rocks and from drift deposits; in Massachusetts the supply from the latter rarely fails. Wells that penetrate into preglacial deposits are largely charged with iron and seldom furnish good water. The till or boulder clay gives good water, except where lime abounds; the water-holding power of these clays is, however, small. Sand plains are favorable for a good water-supply. Professor Shaler gave a detailed description of the sources of the water-supply of southeastern Massachusetts, especially of that of Martha's Vineyard, and in closing said that the desire for pure water was increasing and would shortly be a demand. Boston, from its proximity to the Bristol, Plymouth and Cape sand plains, is favorably situated for an abundant supply, and a reservation of 10,000 acres in the region mentioned would be a benefaction for future generations.

Dr. C. R. Eastman prefaced his paper on some Devonian bone- and fish-beds of North America with an account of the difficulties encountered in the study of fossil fishes, owing to the imperfectness of the material and the lack of systematic exploration. He discussed the structure of *Coccosteus* and described remains of lung-fishes that simulate shark's teeth, found in the Devonian of Iowa.

At the meeting on March 3d sixty-five persons were present. Mr. T. A. Jaggar, Jr., gave an account of his experimental studies of mountain building, illustrating his remarks with a series of models. The scope of geological experimentation was explained, and the conditions under which rocks fracture, the determinant of flexibility and the influence of initial dip noted. A most interesting experiment shows deformation at both ends; the opposite direction of thrusts shows on one side only; the ratio of force to resistance is not influenced by the scale. Mr. J. B. Woodworth remarked upon the geology of the Gay Head Cliff, describing briefly the geological characteristics of the New England group of islands, and giving a detailed account of the formation studied at Gay Head.

A general meeting was held on March 17th; ninety-three persons were present. Mr. Frank Russell gave an account, illustrated by lantern views, of his two years' voyage down the Mackenzie, sketching briefly the characters of the surrounding country and describing with some detail the difficulties that arose owing to the customs and traditions of the Dog-rib Indians. Some of the customs of the Eskimos were noted, also the natural history of the musk ox, Barren ground caribou and bison.

SAMUEL HENSHAW,
Secretary.

THE TEXAS ACADEMY OF SCIENCE.

THE regular monthly meeting of the Texas Academy of Science was held on the evening of April 2d.

A paper on 'Experiments with X-Rays on the Blind' was read by Dr. H. L. Hilgartner, oculist to the State Institution for the Blind. This contribution is the joint production of Dr. Hilgartner and Professor E. F. Northrup, of the chair of physics in the University. As the authors state, the experiments were stimulated by the extraordinary claims made by Dr. Louis Bell in a letter to the editor of the *Electric World* (December 12, 1896), in which it was maintained that a man totally blind from paralysis of the optic nerve was able to distinguish the flickering of a Crook's tube. This the authors wished to verify or disprove. As to their apparatus they say: "The outfit employed in our test is of the best. A double focus tube is excited by a Tesla coil capable of giving an eight-inch discharge. The X-Rays produced will show a shadow of the hand upon the fluorescent screen at a distance of twenty-five or thirty feet." Of the eleven persons experimented upon, seven had no light perception; they were suffering from atrophy of the optic nerve. Of the four having some light perception, three were blind from affections of the cornea and lens, and one from atrophy of the optic nerve. After describing their experiments the authors give as their conclusion "that the X-Rays themselves have no power whatever of exciting vision or even light perception in any kind of an eye, diseased or normal. Of course, these results regarding the blind apply only to the eleven subjects experimented upon, and it would be unscientific to say that no subject can ever be found in whom the X-Rays will excite light sensations. None of the blind subjects could see anything by looking into the fluoroscope, even those having some light perception getting no sensation, and our experiments gave us no hint that the X-Rays, or any other kind of rays, proceeding from the Crook's tube are able to give any light perception to those who are totally blind from any cause whatever." * * * * * * * *

"We should not have thought the above negative results worthy of record if the matter had not been taken up by scientists of eminence and the newspapers filled with trashy and misleading myths."

Mr. J. R. Bailey gave an account of his investigations of the Hydrazine Derivatives of Propionic Acid, being a continuation of his studies begun more than a year ago in the laboratory of Professor Thiele at Munich.

A paper by Mr. M. B. Porter, now of Harvard University, 'On the Roots of Bessel's Functions,' was announced by title.

FREDERIC W. SIMONDS.

UNIVERSITY OF TEXAS.

THE ACADEMY OF SCIENCE OF ST. LOUIS.

AT the meeting of the Academy of Science of St. Louis, held on the 19th of April, 1897, twenty-one persons present. Dr. C. Barck delivered an address on Helmholtz—his life and work; and Dr. C. R. Keyes, the State Geologist of Missouri, presented papers on the relations of the Devonian and Carboniferous systems of the upper Mississippi basin and the distribution of Missouri coals.

WILLIAM TRELEASE,
Secretary.

NEW BOOKS.

The Ancient Volcanoes of Great Britain. SIR ARCHIBALD GEIKIE. London and New York, the Macmillan Co. 1897. Vol. I., pp. xxiv+477, Vol. II., pp. xv+492. $11.25.

The Theory of Electricity and Magnetism. ARTHUR GORDON WEBSTER. London and New York, the Macmillan Co. Pp. x+576. $350.

Elements of Astronomy. SIR ROBERT STAWELL BALL. London, New York and Bombay, Longmans, Green & Co. 1896. Pp. xvi+469.

Numerical Problems in Plane Geometry with Metric and Logarithmic Tables. J. C. ESTELL. New York, London and Bombay, Longmans, Green & Co. 1897. Pp. vii+144.

Topics and References in American History, with Numerous Search Questions. GEORGE A. WILLIAMS. Syracuse, N. Y., C. W. Bardeen. 1897. Pp. viii+176. $1.00.

ERRATA: P. 591, col. 1, line 42 and col. 2, lines 14 and 29 for *Puppis* read ζ *Puppis*. P. 592, col. 2, line 36 for March 19th read March 26th.

Very truly yours
E. D. Cope.